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AN INTERPRETIVE HISTORY OF THIRTY-YEARS (1945-1975) OF WEATHER --ETC(U)

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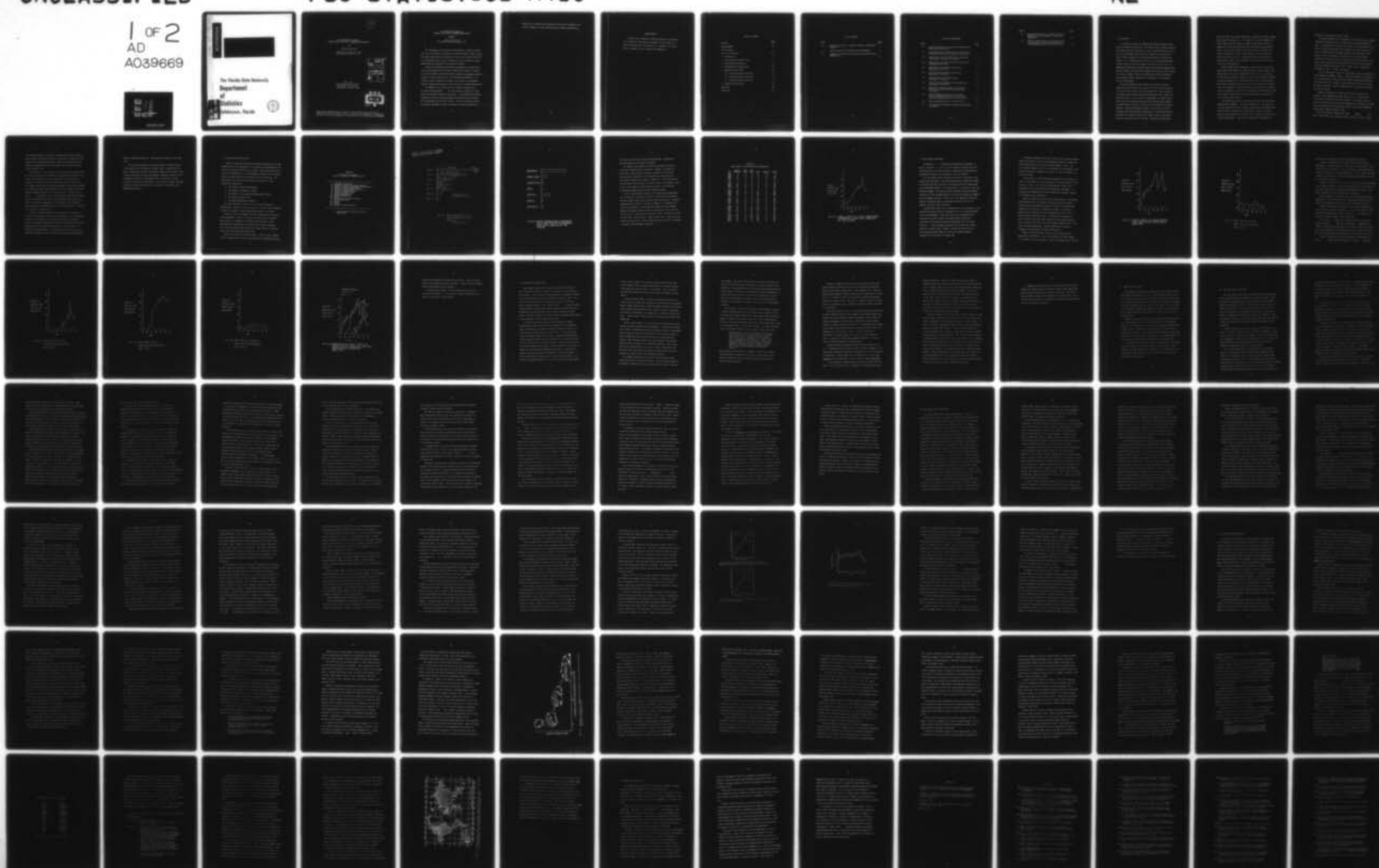

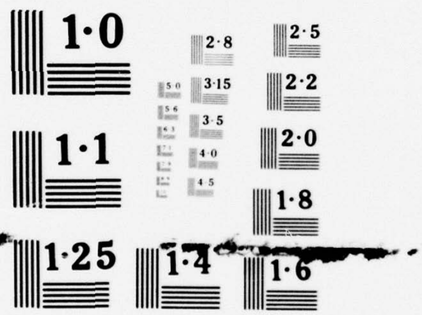

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The Florida State University

Department of Statistics

Tallahassee, Florida

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AN INTERPRETIVE HISTORY OF THIRTY-YEARS (1945-1975) OF WEATHER MODIFICATION

by

Charles Lester Bach

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March, 1977
The Florida State University
Department of Statistics
Tallahassee, Florida 32306

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AN INTERPRETIVE HISTORY OF
30-YEARS (1945-1975) OF WEATHER MODIFICATION

ABSTRACT

Charles Lester Bach
The Florida State University, 1977

The development of the physical understanding of weather modification and the evolution of statistical and meteorological design criteria for weather modification experiments for the 30-year period (1945-1975) are investigated. Also, social, economic,and legal problems of weather modification are discussed as they affect the above.

Graphs are constructed depicting the chronology of reported articles on storm-system (in this paper 'storm-system' refers to attempts to increase rainfall from extratropical cyclones and organized systems of clouds), cold fog, warm fog, hail, and lightning modification. An attempt is made to explain the changes in the number of experiments reported during the description of the evolution of weather modification.

The summary of the 30-year period of weather modification is for three 10-year periods. The first decade is shown to be dominated by scientific innovation and dispute. The second decade featured initiation of efforts by meteorologists and statisticians at ending the controversies of the first 10 years. The third decade was marked by increased exchanges of ideas and results of weather modification

operations at conferences and symposia and serious investigation into social, economic, and legal ramifications of weather modification.

Acknowledgment

I would like to express my sincere appreciation to Professor T. A. Gleeson for his support and guidance in this study. My appreciation goes also to Professors C. L. Jordan, R. F. Perret, and M. A. Hanson for their comments and suggestions.

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1. Introduction

It is useful to begin by differentiating between climate modification and weather modification. The former concerns changes effective over large areas and long periods of time. Melting the Arctic sea ice is one of the possible objectives that scientists speculate about. Calder (1974), suggests the following possibilities of altering the climate: (a) by spreading soot on the ice, (b) by exploding H-bombs to make clouds at the correct height to warm the Arctic; or (c) by damming the Bering Strait between Siberia and Alaska and pumping water into the Pacific to draw the Gulf Stream further north on the far side of the pole. However, even if such objectives could be achieved in the forseeable future, unpredictable by-products of the operation could be catastrophic.

Resulting calamities might include the flooding of heavily populated coastal plains, the turning of fertile areas into deserts, or the destruction of fish, bird, and animal life. Many scientists, for example Mason (1970) and Manabe and Holloway (1970), express the belief that man is not yet ready for experiments in climate control. Mason feels that successful prediction must precede major attempts at modification and control, and here he only foresees very limited advances. Manabe and Holloway conclude that in view of the far-reaching social and economical consequences of climate modification, one should not attempt to modify the climate unless he can predict exactly the results of such an attempt. Despite this belief that

man is not ready for planned modification, inadvertent climatic changes may possibly be taking place. For example, it has been suggested by sundry authors that the release of carbon dioxide (CO_2), from the combustion of coal and hydrocarbon fuel may increase the greenhouse effect. Harris (1973) presents data that indicate the global atmospheric concentration of CO_2 is increasing at a rate of nearly 0.7 parts per million, per year. Combustion products from automobiles which Schaefer (1966, 1973) documents, add ice nuclei to the air and could affect precipitation.

Weather modification encompasses a large variety of activities. These range from alteration of large cyclonic systems to single cloud modification to the use of smudge pots or fans for frost prevention. In this thesis, weather modification refers to the planned alteration of weather phenomena over a limited period of time by cloud seeding. Major goals of weather modification include the increase of precipitation, the dissipation of fog and stratus, the modification of thunderstorms to eliminate or decrease hail, lightning, and strong winds, and the treatment of hurricanes to reduce their intensity, shorten their lives or alter their courses.

From these major goals, one can note the broad area that weather modification encompasses. It would be difficult to cover adequately all these objectives; thus this thesis concentrates on seeding experiments that were concerned with storm-system (attempts to produce rainfall from extratropical cyclones), hail, lightning, warm fog, and cold fog modification. Some types of seeding not considered were

seeding of hurricanes and cumulus clouds.

Hurricane seeding was not included in this study because of special verification problems such as the difficulty of obtaining sufficient data the possibility of harmful side effects on the environment; and, due to unresolved legal and social problems, the need to restrict the area for conducting the experiments (Gentry, 1974 and Tribus, 1970). Another item not considered is individual cumulus cloud seeding; although one should note that much analysis has been done on this topic using classical statistics (Mann-Whitney-Wilcoxon tests) and Bayesian statistics (Simpson and Dennis, 1974). Another type of seeding not discussed is that of Great Lake storms.

Also not discussed in detail are some special types of seeding such as overseeding and dynamic seeding. Overseeding involves an introduction of nuclei into a cloud, which will result in large concentrations of crystals that will be unable to grow sufficiently large to fall out and reach the ground. Often overseeding is not deliberate and may happen when not wanted.

However, overseeding on purpose is often used to suppress the growth of large damaging hailstorms (Mason, 1971). Another example of deliberate overseeding is dynamic seeding. This method involves an alternative approach to precipitation enhancement by releasing the latent heat of fusion and thus sustaining the cloud's circulations (Weinstein and MacCready, 1969 and Woodley, 1970).

Some of the agents considered for cloud seeding are silver iodide (AgI), CO_2, organic materials, propane, and urea. Other

cloud-seeding agents not considered are water-drop seeding, gasoline-engine exhaust, industrial pollution, lead aerosols, ionized air, dust, carbon black, and hygroscopic nuclei. Many of these operate on very different mechanisms and further discussion can be found in Fletcher (1962) and Mason (1971).

Verification of cloud seeding has been done in many different ways. This study considered only the amount of precipitation received at the ground and the diminution of fog. Some areas of verification not considered in detail are radar, stream flows, reservoir runoff, and crop damage. Radar deserves further explanation because this instrument will undoubtedly become very valuable for filling in gaps in the precipitation observing network (if it already has not). However, due to various uncertainties about radar during the time period studied, such as the possibility that the radar measurement aloft may not correlate closely with measurements at the ground, and the absence of a unique intensity relationship (Battan, 1959 and Mason,1971) , I decided not to include a discussion of it.

In 1976, a research proposal by the Department of Statistics at Florida State University listed the following as one of its objectives: To develop a bibliography of statistical methodology and physical, dynamical and synoptic meteorology associated with weather modification experimentation with abstracts indicative of techniques used. On compiling this bibliography, the investigators observed the lack of an interpretive summary or history of both statistical and meteorological

papers on weather modification. This study is an effort to fill this void.

This report concentrates on the modern period of weather modification (after 1930, as defined by Fleagle, 1969). A major effort is made to analyze and interpret this period, especially after 1945, using original papers and the above bibliography. Emphasis is placed on the development of physical understanding and the evolution of design criteria of modification experiments. Also, social, economic, and legal problems of weather modification are discussed as they affect this experimental activity.

2. Data Collection and Data Sets

Table 2-1 lists the main sources of referenced papers used in this investigation of the development of the physical understanding and the evolution of design criteria of this digest. These sources were also used in the compilation of the meteorological abstracts for the bibliography on weather modification mentioned in the introduction. The following are the five main meteorological references of cloud modification experiments:

1. The Journal of Applied Meteorology
2. The Journal of Atmospheric Science
3. The Bulletin of the American Meteorological Society
4. The Journal of Meteorology
5. The Weather Modification Symposia

The list presented in table 2-1 is by no means complete or comprehensive. However, this table represents the most recent summation of sources of papers on weather modification known to the author.

Figures 2-1 and 2-2 represent the world distribution of reported cloud experiments. Figure 2-1 covers the fifteen years, 1946-1960, while figure 2-2 encompasses the fifteen years 1961-1975. These experiments cover modification of extratropical storms, lightning, hail, cold fog, and warm fog, which were the major points of interest in the joint statistics-meteorology study.

Figure 2-1 is taken from Huschke (1963). Figure 2-2 was computed from all relevant articles used in developing the bibliography referred

TABLE 2-1

Main Sources of Papers
Synoptic, Dynamic, and Physical Aspects

1. Atmospheric Technology
2. Australian Journal of Scientific Research
3. Bulletin of the American Meteorological Society
4. Journal of Applied Meteorology *
5. Journal of the Atmospheric Sciences *
6. Journal of Chemical Physics
7. Journal of Colloid Science
8. Journal of Meteorology *
9. Meteorology Magazine
10. Monthly Weather Review
11. Nature
12. The Quarterly Journal of the Royal Meteorology Society
13. Science
14. Tellus
15. Weather
16. Weather Conference Symposia *

* - Main References of Cloud Modification Experiments

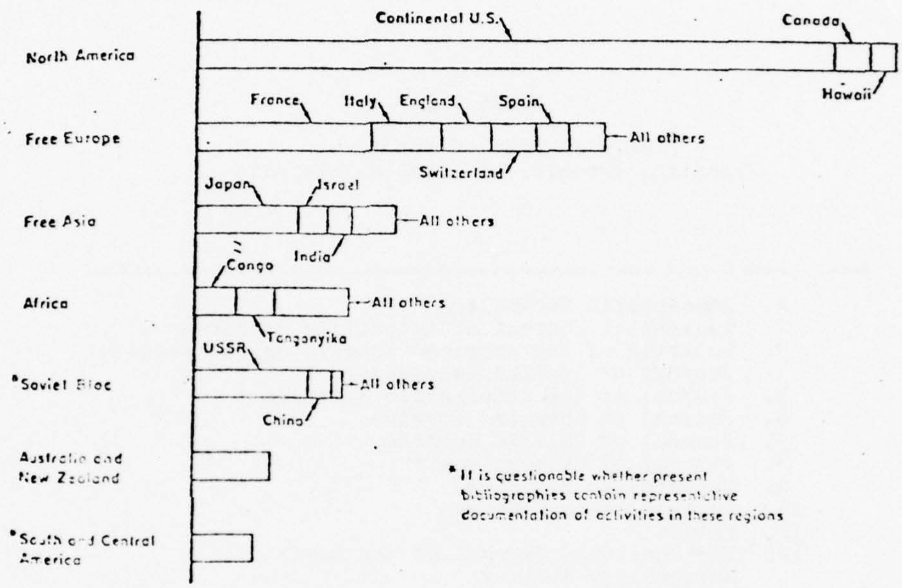

FIG. 2-1. WORLD DISTRIBUTION OF REPORTED CLOUD MODIFICATION EXPERIMENTS (1945-1960).
FROM HUSCHKE (1963)

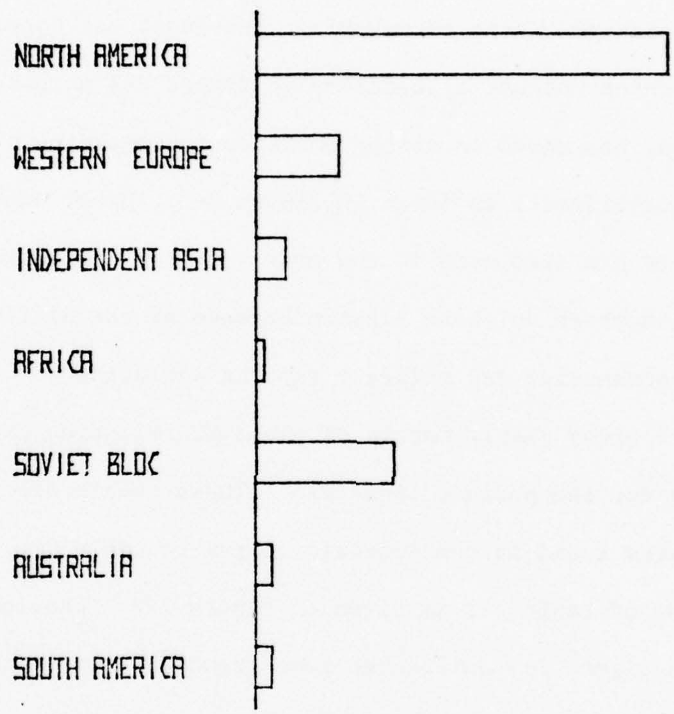

FIG. 2-2. WORLD DISTRIBUTION OF REPORTED CLOUD MODIFICATION EXPERIMENTS (1961-1975), COMPILED BY THE AUTHOR.

to earlier, and was constructed following Huschke. Similarities and differences occur in these two figures.

For example, North America in both time periods has reported the largest relative amount of material. However, one notes the Soviet Bloc, which Huschke illustrates in figure 2-1 as being in fifth position, has moved to second position in reporting cloud modification experiments as shown in figure 2-2. Other similarities and differences are discussed in the next section. No attempt is made to put actual numbers on these figures because of the difficulty of obtaining commercial and military reports worldwide.

Table 2-2 gives yearly totals of cloud modification experiments for all areas for the period, 1945-1975. These totals are compiled from the reports found in the journals listed in table 2-1. Graphical representation of table 2-2 is given in figure 2-3. Examination of table 2-2 and figure 2-3 inidicates some trends and major changes. General, increasing trends are noted for the periods 1945 to 1949 and 1951 to 1964. A large increase occurred at 1969. Decreases are noted at 1950, 1964, and after 1970. Finally, after 1972 a leveling off and slight increase occurs. These increases and decreases will be examined in section 5 and discussed in section5.

TABLE 2-2

Yearly Totals of Cloud Modification Experiments

Year	Extra-Tropical	Cold Fog	Warm Fog	Hail	Lighting	Totals
1945	1	2	0	0	0	3
1946	2	3	0	0	0	5
1947	6	4	0	0	0	10
1948	8	4	0	0	0	12
1949	11	4	0	0	0	15
1950	7	2	0	0	0	9
1951	13	2	0	1	0	16
1952	10	4	0	1	0	15
1953	14	4	0	3	1	22
1954	13	4	0	3	1	21
1955	13	4	0	3	1	21
1956	12	2	0	13	1	28
1957	17	2	0	14	3	36
1958	16	3	1	15	3	38
1959	17	3	1	17	3	41
1960	19	3	1	18	2	43
1961	21	2	5	18	2	48
1962	23	5	1	20	3	52
1963	25	5	3	23	2	58
1964	26	6	5	22	2	61
1965	14	6	5	21	3	49
1966	17	2	9	22	3	53
1967	18	3	10	24	2	57
1968	19	3	8	24	2	56
1969	21	3	20	25	2	86
1970	26	2	11	23	2	64
1971	23	3	12	22	3	63
1972	13	1	10	21	3	48
1973	12	2	8	22	2	46
1974	15	3	7	22	1	48
1975	16	3	8	23	1	51

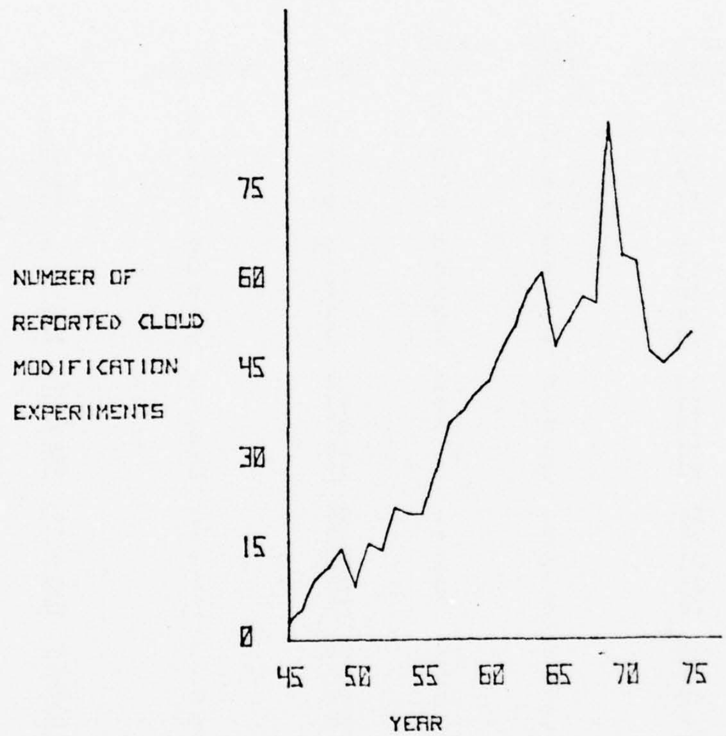

FIG. 2-3. YEARLY TOTALS OF CLOUD MODIFICATION EXPERIMENTS (1945-1975), COMPILED BY THE AUTHOR.

3. Data Analysis and Results

In addition to the similarities and differences of figures 2-1 and 2-2 as noted in section 2, it is of interest to observe the relative decrease by all other countries in reporting cloud modification experiments as shown in the comparison of the two figures.

One can surmise several reasons for the shift in reporting cloud modification experiments. The World Meteorological Organization (WMO), in Technical Note #146, discusses the present position of meteorology in the developing nations (in a broad sense in the tropical zone), and desirable changes. The report provides essential social and economic statistics for the six WMO Regions of the world and divides its members into two separate sections: Group A (34 of 137 members), which have reached an advanced stage of development and Group B (103 of 137 members), which are still developing.

The WMO report notes that the population of the 103 members of the developing countries (Group B) represents 72% of the total population of the 137 WMO members. This group shares the responsibility for observations in their networks and meteorological studies for continental territories and islands representing 59% of the total land area. This percentage increases if one extends the responsibility to oceanic areas. However, the WMO also notes that the Gross National Product (GNP) of the 103 less advanced members represent 17% of the total 137 member GNP.

Alternately, Bernard (1975) notes that in 1971 the poorer members spent \$92 million on all their meteorological services as against the \$543 million spent by the 34 more affluent members. Thus, 85% of the funds available in 1971 for meteorology was concentrated in the developed countries. Whatever the reason, one can see economics is a definite problem.

Yearly totals of cloud modification experiments are presented in table 2-2 and figure 2-3. One sees general increases from 1945 to 1949 and 1951 to 1964. A very large increase appears in 1969. Also a leveling off occurs at 1973 with slight increases afterwards. To facilitate the examination of these increases and decreases, figures 3-1 through 3-6 were constructed.

Figure 3-1 shows the yearly totals of storm-system (in this paper storm-system refers to attempts to increase rainfall from extra-tropical storms and organized systems of clouds) cloud modification experiments for 1945-1975. Examination of figure 3-1 reveals a general overall increase with minor decreases from 1945 to 1964. A major decrease occurs about 1965. This decrease is followed by a rapid increase to previous highs about 1970 with decreasing amounts after this time, till 1973. After 1973 slight increases occur. This figure indicates storm-system modification has been prominent throughout the development of weather modification.

Figure 3-2 shows the yearly totals of cold fog modification experiments for 1945-1975. There is no indication of large changes in the number of cold fog projects. Hence it is apparent that cold fog

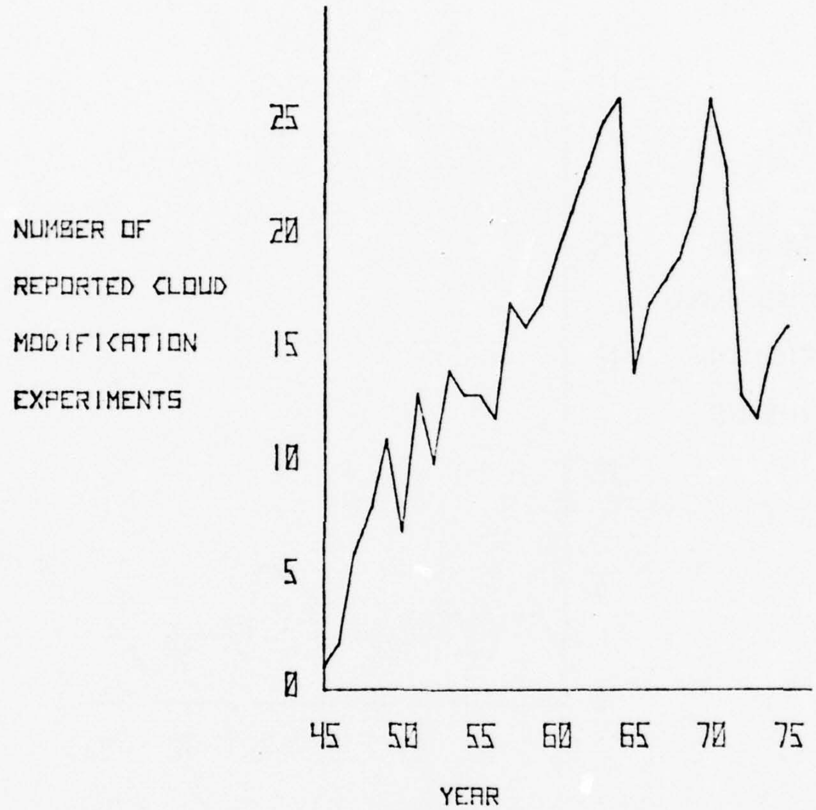

FIG. 3-1. YEARLY TOTALS OF STORM-SYSTEM CLOUD MODIFICATION EXPERIMENTS (1945-1975).

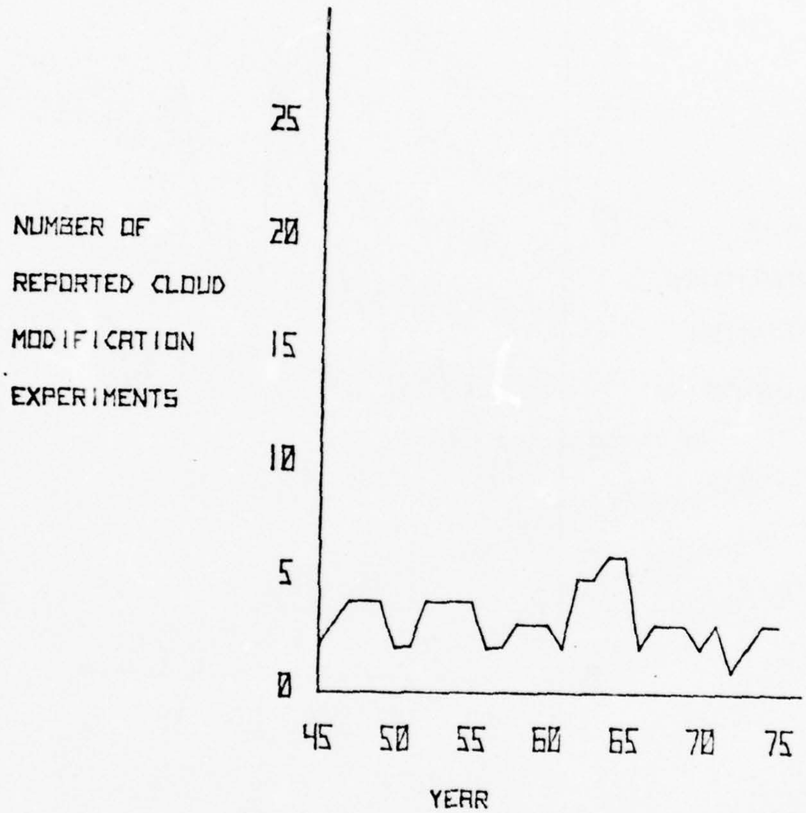

FIG. 3-2. YEARLY TOTALS OF COLD FOG CLOUD MODIFICATION EXPERIMENTS (1945 - 1975).

has had only minor affect on total cloud modification trends.

Figure 3-3 shows the yearly totals of warm fog modification experiments. From this figure, one sees very little was done in this field till the late 1950's. By the late 1960's, large increases had occurred in this area of weather modification. These changes definitely affected the total amount of weather modification experiments and will be scrutinized in section 5.

Figure 3-4 shows the yearly totals of hail modification experiments. As this figure indicates, this type of experiment had a large affect on the total amount of cloud modification experiments after 1955. This large increase will also be studied later in section 5.

Next is figure 3-5, the yearly totals of lightning modification experiments. As seen from figure 3-5, lightning modification has had very little significant effect and will not be examined in detail.

Finally, from these five figures we see that storm-systems, hail, and warm fog are the major elements in the overall reported cloud modification experiments as shown in figure 2-3. For comparison of these three figures, figure 3-6 was constructed.

This gives a comparison of yearly totals of storm-system warm fog, and hail modification experiments. Examination of figure 3-6 indicates the major part of the large increase around 1969 in the total reported cloud experiments as shown in figure 2-3 to be due to the unusual number of warm fog articles reported for that year.

Figure 3-6 also illustrates the chronology of modification for three modification experiments as follows. First was

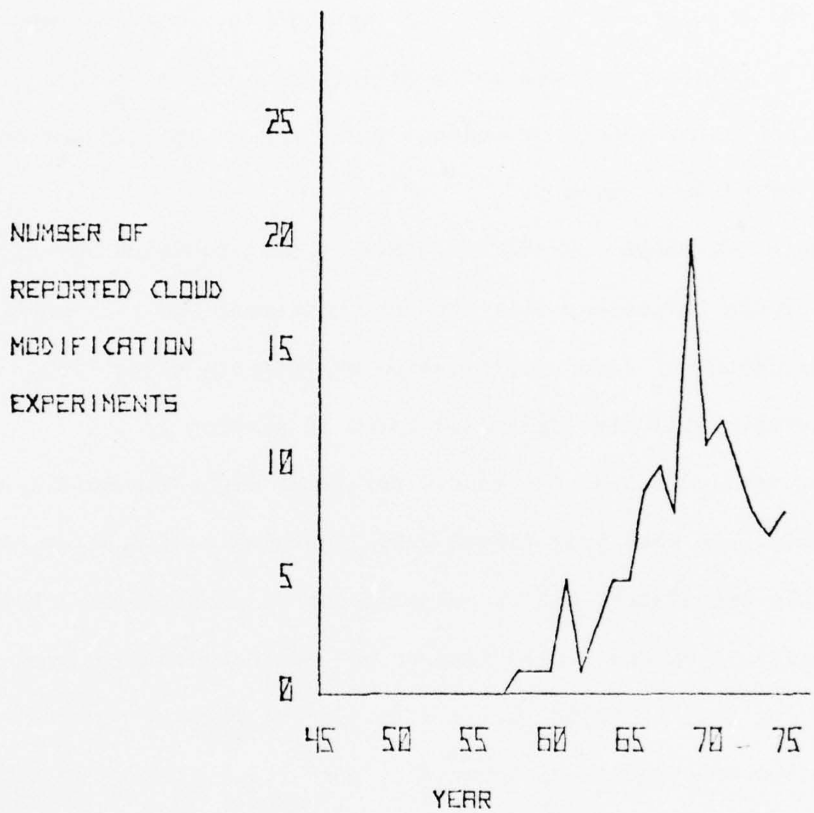

FIG. 3-3. YEARLY TOTALS OF WARM FOG CLOUD MODIFICATION EXPERIMENTS (1945 - 1975).

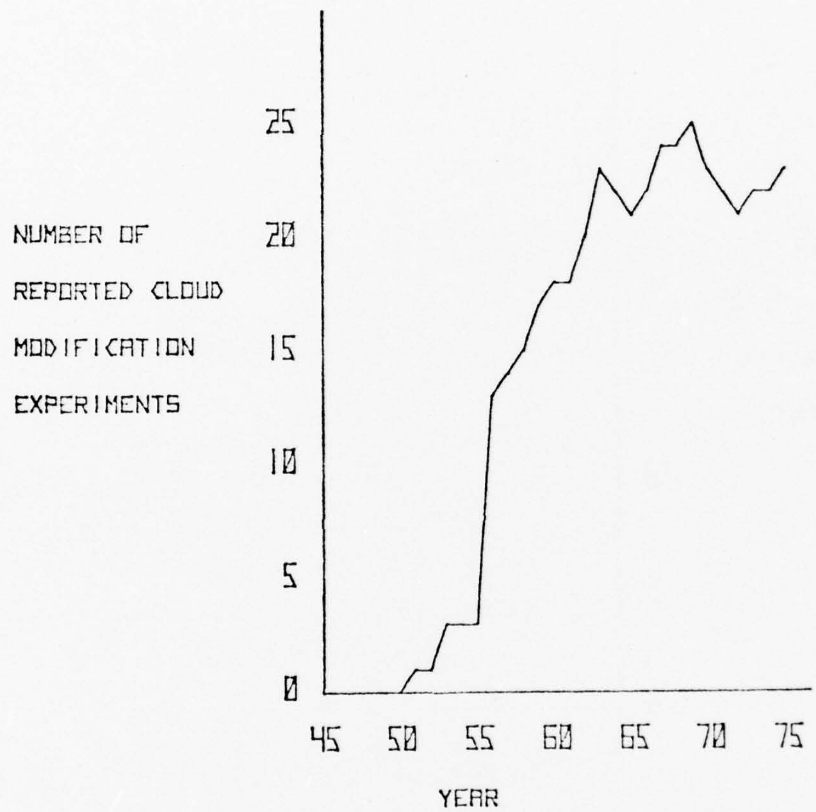

FIG. 3-4. YEARLY TOTALS OF HAIL
CLOUD MODIFICATION EXPERIMENTS
(1945 - 1975).

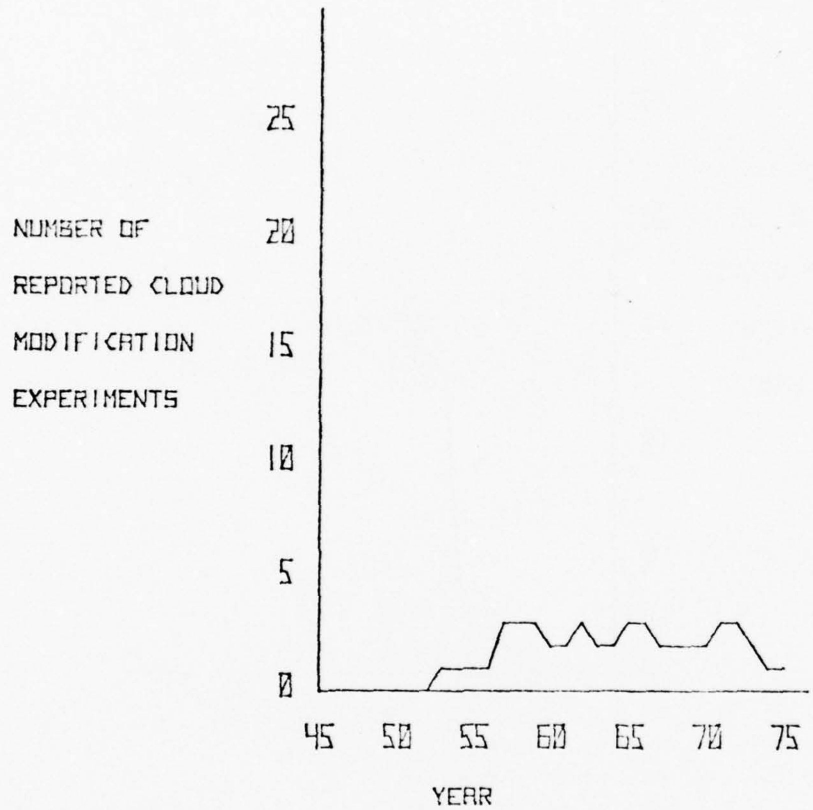

FIG. 3-5. YEARLY TOTALS OF LIGHTNING CLOUD MODIFICATION EXPERIMENTS (1945 - 1975).

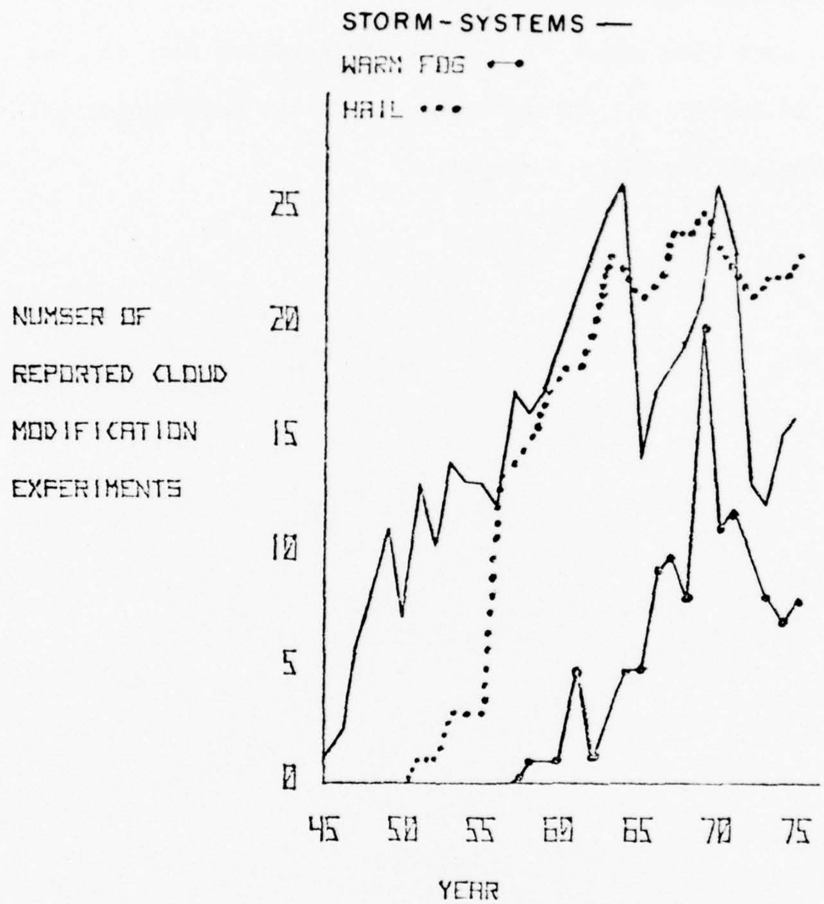

FIG. 3-6. COMPARISON OF YEARLY TOTALS OF STORM-SYSTEM, WARM FOG, AND HAIL MODIFICATION EXPERIMENTS (1945-1975).

storm-system modification around the late 1940's. Then in the mid-1950's, hail modification became important. Finally in the late 1960's warm fog modification became important.

There have been trends in weather modification that will be discussed in section 5. First, however, a short description will be given of the history of this subject.

4. Pre-Modern Era (before 1930)

Since ancient times, man has sought to influence the weather. Man's aspiration to control or modify the weather is deeply anchored in antiquity. The Old Testament prophet Malachi, for example, promised that tithe bearing would cause "the windows of heaven to open." The stories of the Great Flood of Genesis and the plague of flood and storm in Exodus establish a time reference here. Despite failures, the aspiration to change weather emerges again and again in folklore, in Biblical and other proverbs, and in ancient festivals and rain dances. One must recognize that modern interest in weather modification has an extensive, if undistinguished, ancestry.

In recent history many people who were prepared to dismiss incantations to gods, believed that the weather could be changed by noise and explosions of gunpowder. During the Napoleonic Wars it was noted that after large battles it often rained. It was argued that cannon and musket firings were the cause. Subsequent investigations convinced almost everyone that this interpretation of the rain following the battle was not as many had presumed. As Battan (1962) noted, rain often followed battles anyway. The explanation of this result is simply that the battle was fought before the rain. In the days of Napoleon, troops and guns were transported by horses and wagons. It was necessary that the ground be dry in order to move swiftly. As a result, the generals planned their attacks for periods of dry weather.

As we all know, however, the general situation outside desert and arctic regions is that dry periods may last for a few days, possibly a few weeks, but sooner or later it rains again. Thus, the reason wet days often followed battles was that nature was taking its usual course.

Later during the 1800's, considerable attention was devoted to understanding the clouds, the atmosphere, and processes of precipitation. Khrgian (1970) observed that in 1803 Luke Howard developed a seven-type cloud classification featuring cirrus, cumulus, stratus, cirrocumulus, cirrostratus, stratocumulus, and nimbostratus. Middleton (1965) noted that Dalton found a relation between saturation vapor pressure and temperature.

Later in this century, it was found that cloud droplets form only on small particles suspended in the atmosphere. W. Hess (1974) remarks that in 1875, a Frenchman named Coulier published results of experiments demonstrating that particles floating in the air served as nuclei on which condensation could occur with little or no supersaturation. Aitken in 1881 followed Coulier's work with equipment that produced supersaturations in which the most numerous particles, those between 0.01 and 0.1 microns radius, are active. Weather modification from those days until the present has provided a shifting field in which strongly held opinions have been vigorously contested.

For example, during the first half of the nineteenth century conflicting theories of storms developed. The vortex theory advanced by William C. Redfield and the convection theory by James P. Espy was

one example. Espy (1841) understood the role played by stability in vertical convection and made quantitative calculations based on this mechanism. His enthusiasm led to the view that all precipitation and other aspects of storms are to be explained as direct consequences of convection and the release of latent heat of condensation. Espy's views are now recognized as extreme, but they contained important elements of the mechanism of storms, and they attracted considerable attention.

Espy served as an advisor to Congress on meteorological problems. In 1850 he proposed what may be the first federally financed scheme for large-scale weather modification. Espy suggested that forty-acre masses of timber in the western states be set afire simultaneously. This was to be done every twenty miles along a north-south line of six- to seven-hundred miles, at seven-day intervals. The entire holocaust would, he believed, initiate a "rain of great length" that would:

> "travel toward the east side-foremost; that it will not break up until it reaches far into the Atlantic Ocean; that it will rain over the whole country east of the place of beginning; that it will rain only a few hours at any one place ... These, I say, are the probable-not certain results of the plan proposed-a plan which would be carried into operation for a sum which would not amount to half a cent a year to each individual in the United States."[1]

The proposal was not endorsed by Congress. Not because of doubts concerning Espy's meteorology, but because Congress had not then become accustomed to appropriating large sums for scientific or technological enterprises.

Interest in weather modification in the second half of the nineteenth century is attested to by the fact that weather modification patents were granted to many people. Three examples are: a patent to General Daniel Ruggles in 1880 for making rain by explosions in clouds; one to J. B. Atwater in 1887 for dissipating tornadoes by detonating explosives in their centers; and one to L. Gathman in 1891 for initiating rain by exploding a shell containing "liquefied carbonic acid gas" at cloud height.

Faith and interest in efforts to influence weather were sufficient so that in 1890 the United states Congress first granted $2,000, then $7,000, and finally $10,000 to support experiments to be carried out under the Department of Agriculture. These were the first federal attempts to support weather modification. The experiments were carried out by General (self-styled) Robert St. George Dyrenforth, first over Washington, D.C., and later in Texas. He used a variety of "explodents," detonated singly and in volleys, on the ground and aloft. Accounts in the New Yorker magazine described it as quite a show but quite unsuccessful in producing rain.

Perhaps the most picturesque of these "weather modifiers" was J. S. A. MacDonald, alias "Colonel Stingo, the Honest Rainmaker." He applied his considerable knowledge of probability to the uncertainties of rainmaking. Colonel Stingo was the inspiration for a play, The Rainmaker, by N. Richard Nash, and for a musical, 110° In The Shade.

In North America, rainmaking pushed forward with great zeal near 1920. One of the most prolific "rainmakers" of North America was Mr.

Charles M. Hatfield. Hatfield's method was to mix up a batch of unknown chemicals and claim the mixture would "draw clouds from other parts" or simply open up clouds that were already present (Jefferies, 1921). Hatfield could hardly be called a scientist, in fact, his method was only attempted when under contract for financial reward. In 1921 alone, Hatfield was under contract to the communities of Medicine Hat, Alberta; Ephrata, Washington; Milwaukee, Wisconsin; and San Diego, California (Rheingrover, 1975). In San Diego, he had obtained a contract which would pay him $1000 if sufficient rain fell to fill the city reservoirs (Brooks, 1920).

There were similar weather modification efforts at about the same time in Europe. Church bells were rung in an effort to ward off evil spirits responsible for the damaging hailfalls in Central Europe. In 1896, an Austrian burgomaster (Mayor) set up an array of thirty-six specially equipped "hail cannons." The hail cannon consisted of a vertically pointing three-centimeter mortar above which was suspended the smokestack of a steam locomotive. This device not only produced an appalling sound, but created a smoke ring a meter or so in diameter. This smoke ring ascended at about one hundred feet per second and produced a singing note lasting about ten seconds. The initial trials were quite successful as rain appeared and the hail cannon was widely adopted. After numerous deaths and injuries due to the cannon, the Austrian government called an International Conference in 1902 to assess the effects of the hail cannon. The conference proposed tests, the results of which thoroughly discredited the device.

To summarize the pre-modern era, we see that the first serious attempts at weather modification appear to have occurred about 1870. These attempts involved kites, balloons, projectiles, loud noises, and even smoke to induce cloud formation or to produce rain. Few of these efforts were based on sound physical principles and none passed the test of practical acceptance.

5. Modern Era (post 1930)

This chapter concentrates on the post-1930 era of weather modification (WM) with emphasis after 1945. Fleagle (1969) defined this to be the modern era of WM because the basis for artificial modification of clouds was established in 1933 and 1938. This was when Tor Bergeron and Walter Findeisen advanced a theory of rain based on the coexistence of ice crystals and supercooled droplets in clouds at the same temperature, which involved the rapid transfer of vapor from the droplets to the crystals.

However, it was not until 1946, when Vincent Schaefer discovered that a tiny fragment of dry ice, when dropped into a cold chamber, resulted in the formation of ice crystals, and Bernard Vonnegut showed a similar effect using silver iodide particles, that actual cloud modification was done. Because Schaefer's and Vonnegut's discoveries did not occur till 1946, many meteorologists, (for example Byers, 1974; and Petterssen, 1969) define the modern era of WM as after 1945.

The next three sections interrelate the evaluation of the physical understanding and the evolution of design criteria of WM. Social, economic, and legal problems are examined also, as they relate to the development of WM. The first section starts with late 1945 and describes the next ten years.

5a. The First Decade (1946-1955)

The first decade of WM, which in effect started the development of this field, extended from Schaefer's cold-box experiment in 1946. This decade was characterized by scientific innovation and dispute, commercial exploitation, absence of statistical design criteria, and legislative inaction. The discussion will deal primarily with the scientific discoveries and arguments which dominated this 10-year period. Following the discoveries of Schaefer and Vonnegut, a mass of scientific literature on WM technology was published. The majority of these articles were concerned with silver iodide (AgI) and its properties.

Conflict covered a wide realm of topics the first decade. In a span of a few years, novel ideas were redefined, repudiated, or both due to the lack of cloud physics knowledge. Some areas of innovation and disagreement included the ice nucleating ability of AgI, the economic feasibility of cloud seeding, the use of CO_2 for relief of drought, and differing opinions of rain mechanisms.

The ice-nucleating ability and decomposition of AgI debates resulted in the largest number of publications compared with any other area of research of WM. The most important discovery about AgI was made by Vonnegut (1947). He found that when AgI smoke was introduced into a supercooled cloud in the laboratory, some ice crystals appeared when the temperature fell below -4C and that the numbers increased with decreasing temperatures until, at about -15C, most of

the AgI particles appeared to serve as nuclei. Vonnegut believed that AgI served as very effective nuclei because it closely resembled ice in crystal structure. The success of AgI stimulated an intensive search for substances which might be more efficient than AgI. Also, other investigations were initiated into the physical properties of AgI.

Three examples of this search for inorganic substances during the first decade are from the following scientists: Aufm Kampe and Weickmann (1951) who examined silver iodide, cadmium iodide, and cobalt; Schaefer (1954) who investigated silver iodide and lead iodide; and Mason and Hallett (1956) who experimented with thirty substances including silver iodide.

Aufm Kampe and Weickmann did measurements of natural and artificial freezing-nuclei in a room-size cold chamber. Their investigations of silver iodide, cadmium iodide, and cobalt iodide indicated that cobalt iodide was almost effective a freezing nucleus as silver iodide. However, they noted that due to cobalt's high hygroscopicity (ability to absorb moisture from the air), it probably could not be used to seed clouds from the ground. Aufm Kampe and Weickmann also believed seeding effects would be local and of short duration. This negative opinion illustrates a pessimistic view of WM held among research meteorologists which is examined later in this section.

Schaefer found both AgI and PbI served with equal effectiveness as sublimation nuclei at temperatures colder than -5C. Mason and Hallett stated the ice-nucleating ability to be greatly inhibited if the

AgI crystals were irradiated with ultra-violet (UV) light. These three articles not only point out the altercation over ice-nucleating ability, but also illustrate two other conflicts over AgI.

These two disputes were about the deactivation of AgI by UV light and whether AgI nucleates as a freezing or sublimation nucleus ("freezing nuclei" are defined as a special form of ice nuclei which nucleate the liquid phase and "sublimation nuclei" as ice nuclei which nucleate the vapor phase in starting the growth of an ice crystal, AMS, 1968).

By following the AgI deactivation controversy, one can see that scientific innovation and dispute occurred throughout this decade. The issue of the role of AgI as a freezing or sublimation nucleus is discussed further in the next section. The following not only illustrates the dispute of this period, but also indicates a beginning in the change of attitude about WM design especially in the second half.

During 1951 many papers were written about the effect of sunlight on AgI. Reynolds et al. (1951) and Inn (1951) both reported sunlight had a deleterious effect on AgI. Reynolds et al. reported that the number of nuclei in a given sample of smoke was reduced by a factor of from ten to one hundred after a one hour exposure to sunlight or comparable UV radiation. Inn said that when AgI nuclei were exposed to light for 20 minutes or more, the ability to form ice particles when injected into a cloud of supercooled water droplets was found to essentially be destroyed. On the other side were Vonnegut and Neubauer (1951), who found UV light deactivates AgI much less rapidly than others had observed. They noted 40 to 100% of the AgI nuclei

remain active after a one hour exposure to UV.

Innovation also occurred during this time. Reynolds et al. (1952) noted the concentration of effective nuclei was greatly increased by the addition of a little ammonia. Birstein (1952) added to the general controversy of this period with his experiment that showed the effectiveness of AgI exposed to UV light was directly dependent on the relative humidity of the gas stream passing over the generator.

Other examples of the AgI dispute were expressed by Bolton and Qureshi (1954) who reported the decay rate of AgI to be critically dependent on the ambient air temperature and to a lesser extent on the air pressure. In 1955, Smith, Heffernan, and Seely (1955) did free atmosphere tests. They found the total number of AgI freezing nuclei, effective at -17C, decreased by a factor of 10 after eight minutes of exposure in the atmosphere. They also differed with Birstein as noted earlier by observing that the rate of decrease of the effectiveness of AgI was not influenced by humidity.

Accompanying the AgI debate of this decade, were a variety of suggestions for modification activities. Reynolds et al. (1951) observed that the extensive employment of AgI in numerous commercial efforts at artificial nucleation evidenced the necessity for determining its rate of decay under expected conditions of radiation in the free atmosphere. This suggestion led to many papers as noted earlier.

Bolton and Qureshi (1954) and Smith, Heffernan, and Seely (1955) made suggestions for rainmaking. Both followed similar physical

reasoning, and argued that for AgI to be effective it must be released at a relative cold temperature. Bolton and Qureshi suggested AgI may be effective when released from high mountains or aircraft. Smith, Heffernan, and Seeley suggested that for AgI to be effective, it must be distributed through the layer of the atmosphere where temperatures were about -10C to -15C. These suggestions were put into design criteria for WM projects as evidenced by the large number of aircraft seeding operations.

As the decade closes on the silver iodide dispute, one sees a new direction of the investigation of the irradiation of silver iodide. The new approach was aimed at the surface chemistry of AgI. Birnstein (1956) made studies of the adsorption (adhesion of a thin film of liquid or gas to a solid substance) of water vapor in photolyzed AgI. He characterized water on AgI as having extremely high multilayer adsorption and as AgI was photolyzed its nucleating ability and water adsorption were greatly diminished. The next decade would see much investigation into the surface chemistry of AgI. AgI was not the only seeding agent in the dispute of this period as we see in the following discussion of CO_2.

Schaefer's (1946) discovery of CO_2 as a seeding agent, and his subsequent atmospheric tests on stratiform clouds, led him to the suggestion that cloud seeding could be economically feasible. Coons, Jones, and Gunn (1949) did experiments with CO_2 that showed artificial modification of cumuliform clouds was of doubtful economic importance for the production of rain. Dissipation rather than new development

was the rule they expressed. They believed these methods were certainly not promising for the relief of drought.

Not all expressed this negative view of CO_2. Frith (1950) gave positive reports of attempts to seed clouds with CO_2 during an extended drought. Squires and Smith (1949) reported on 20 experiments in Australia using CO_2 of which 15 clouds precipitated.

However, the popularity of CO_2 has diminished over the years (Henderson, 1972). He observes that a portion of this rejection can be attributed to storage problems, a short shelf life, processing difficulties, and the requirement for relatively large volumes of this material. Thus, a major portion of nucleation research was directed away from CO_2 and toward the generation of AgI and other particles, as discussed in the next section.

Rain mechanisms were one meteorological factor where there was redefinition instead of repudiation of previous results. Vierhart (1950) did calculations of Langmuir's chain-reaction theory and suggested reasons for "raining out" of clouds. Houghton (1950) redefined the two principal mechanisms for formation of precipitation: (1) the Bergeron-Findeisen ice-crystal process and (2) the collision (or coalescence) of particles of unlike size in the gravitational field. Telford (1955) suggested that rain forms much faster by the coalescence theory than expected before.

The bickering of this decade did not hinder considerations for physical and statistical design criteria. The beginning of the period was dominated by programs aimed at physical evaluation. By the middle

of the period a trend toward statistical analyses and then toward statistical design criteria developed.

The initial attempts at seeding were exploratory. Examples of this attitude were the following: the Cloud Physics Project and Project Cirrus in the United States; and the field experiments in Australia as reported by Kraus and Squires (1947), Smith (1949), and Squires and Smith (1949).

An interesting point observed by Court (1967) was that Project Cirrus did not even consider randomization and the Cloud Physics Project rejected it as unnecessary in view of the control exercised by the use of the several observational aircraft and, especially, of a rain sensitive radar!

A changing trend in evaluation of WM was expressed by Vonnegut (1950) who did a "try it and see what happens" experiment showing clear positive results of AgI seeding. Important was his suggestion that modification results be studied by statistical analysis of weather data.

Statistical analyses slowly developed in WM after this suggestion. Langmuir (1950) used regression analysis, F-ratios, and correlations in an attempt to detect a seven-day periodicity in the weather at significant distances introduced by a AgI generator in New Mexico. One should note that seven-day cycles have been observed in the weather (Lewis, 1951; and Wahl, 1951) as well as other cycles (Lamb, 1972; and Stringer, 1972a, 1972b). Brier and Enger (1952) employed regression analysis and scatter diagrams to do a more complete analysis of the

1951 Arizona cloud-seeding experiment. Elliott and Strickler (1954) used scatter diagrams and linear regression to study the effects of seeding on water shedsin California, Oregon, and Idaho. This movement for statistical evaluation increased in magnitude during this decade. However, even slower to be incorporated into WM were statistical design criteria.

Lambright (1970) put part of the blame for this slow evolution on meteorologists. Their general attitude for this decade, he notes, was that rainmaking was in a basic research state and that claims of success in augmenting precipitation were more often than not disproved.

Lambright feels that the extreme skepticism(he characterizes it as negativism) of meteorologists over the years toward rainmaking probably stems in part from the way the WM entered the field of meteorology. He observes WM as pushed upon the meteorological community from "outside," that is, by non-meteorologists such as Langmuir, a chemist. Meteorologists dismissed Langmuir's claim of large scale weather effects due to cloud seeding and responded defensively to his claim by showing why cloud seeding could not have the effect that Langmuir thought. Two examples of the meteorologist's negative position as noted earlier were noted by Aufm Kampe and Wieckmann (1951) and Coons et al. (1948), who doubted the large scale effects of WM and the economic importance of WM, respectively.

Also, the private sector, according to Lambright, did not promote the statistical-design-criteria evolution during this period. He calls attention to the fact that one of the first to adopt Langmuir's tech-

niques was Irving Krick, who was already suspect among the leaders of his profession for his long-range forecasting. When Krick became involved and rainmaking became big business, many meteorologists wrote off the field from science (Lambright, 1970; and Price, 1965). Meteorologists concentrated on research instead of the operational aspect and a result was a lack of suggestions for operational statistical-design criteria.

In addition to the meteorologist's pessimistic view, the federal government did little more than supply monetary support. The only major legislation during these 10-years occurred in August, 1953. Then the Congress created merely a National Advisory Committee on Weather Control to investigate the extent to which the United States should engage in weather modification research or regulate weather modification activities. The committee recommended only further meteorological research but no organization to direct or fund it. Also the committee did not push for federal legislation governing the use of the technology or as Court (1967) noted, for a scientifically-designed, randomized cloud seeding experiment.

One area meteorologists did concentrate on during this decade was physical design criteria. Frith (1949) made an early call for the development of cloud physics and knowledge of cloud drop-size distributions. Coons, Jones, and Gunn (1949) found seeding effectively shortened the aging process of cumulus and apparently inhibited the growth of clouds by initiating premature downdrafts of ice crystals which subsequently choke off necessary inflow at lower altitudes.

Braham, Reynolds, and Harrell (1951) assumed that the reason some supercooled clouds do not precipitate was that an insufficient number of ice particles was present. They stated that the determination of the relative abundance of such supercooled clouds would have great bearing on the potentialities of increasing precipitation by cloud seeding. Smith and Heffernan (1954) showed from results of measurements in the free atmosphere, that practical rainmaking would require large numbers of nuclei.

In 1951, the Compendium of Meteorology was published. The section of this book on cloud physics emphasized the need for more laboratory and free atmosphere experiments. Junge (1951) called for the development of a new method for the measurement of the entire spectrum of nuclei. Coons and Gunn (1951) believed that further progress in cloud modification will depend upon the development and invention of better airborne instruments suitable for making rapid determinations of the detailed characteristics of clouds. Finally, Houghton (1951) noted that our understanding of the physics of condensation and precipitation was incomplete in many areas and suggested experimentation into (1) the factors determining the breadth of the drop-size distribution; (2) the knowledge of the ice phase; (3) the nature and mode of action of freezing nuclei and sublimation nuclei; and (4) the study of growth of drops by collision in the gravitational field. Meteorologists by the end of the decade started to examine areas of design and evaluation other than the physical one.

Decker (1954), in a report to the American Meteorological Society (AMS) believed two questions must be answered when evaluating attempts at weather control: (1) What actually occurred?; (2) What would have occurred naturally in the absence of weather control? Decker believed the first question could be answered satisfactorily by a dense network of stations and the second by frequent instrumental measurement.

Before concluding the discussion of this period, one should recall that, partially due to the negative attitude of some meteorologists, only physical design criteria on extratropical clouds had been emphasized. Also, other areas of modification had not developed at this time. This emphasis resulted in a relatively large number of experiments in this area and thus, as shown in figures 3-1 and 3-6, storm-system modification was the largest and most important area of storm-investigation during this decade.

We have seen that this ten year span was dominated by discovery and conflict, commercial exploitation, lack of statistical design criteria, and legislative inaction. However, also noted was a changing attitude among meteorologists about directions of research concerning seeding agents and about the need for operational statistical design criteria. The next section will examine the changing attitudes during the second decade.

5b. The Second Decade (1956-1965)

The second decade of WM featured initiation of a variety of evaluation efforts aimed at ending the controversies of the preceding era. Evaluation became a dominant factor in WM after the 1957 report of the U.S. Advisory Committee on Weather Control. The committee further recommended that the National Science Foundation (NSF) be the focal point to promote and support WM research. With this committee's encouragement, some grand and diversified attempts to evaluate WM field projects caused meteorologists to recognize the extent to which atmospheric processes remained physically undefined, and therefore, physically and statistically unpredictable. Huschke (1963) observes that some of these grand attempts were Projects Overseed, Skyfire, Seabreeze, Sailplane, and some aspects of the Santa Barbara Projects which started the trend back to investigation of the scientific bases of WM. The federal government showed interest by passing legislation, during this time to maintain and stimulate the activity. Hail suppression and research using organic materials as seeding agents became important during this span. This decade ended with a report of the National Academy of Sciences (NAS) which was published in 1966, and with uncertainty in WM policy, as detailed in the present section.

Evaluation attempts in the form of three randomized experiments began early in the second decade of WM research. The first sought to determine the magnitude of precipitation increase from ground release of AgI into winter storms on California's coast, the other two both

involved summer cumulus clouds. In Arizona the clouds were treated from the air to study precipitation processes and increase mountain runoff, and in northeastern California the clouds were treated from ground in hopes of reducing lightning on dry forests.

The Santa Barbara project on California's coast ran from January through May, 1957-1960. Court(1967) notes that this was the first randomized project in which the target was fixed. The target was the entire county of Santa Barbara (5478 sq. km.) divided into subtargets in which were placed about 50 recording raingages. Randomization was based on twelve hour periods. Despite intensive efforts at data collection and careful statistical analysis, results were inconclusive at best as reported by Neyman, Scott, and Vasilevskis (1960).

Also begun in 1957 was what was to become a seven-year program of aerial seeding of summer cumulus clouds over the Santa Catalina Mountains, east of Tucson, Arizona. Randomization was attained using pairs of days that had been declared seedable by an objective criterion. Precipitation was measured by meteorologically defined targets, on the basis of wind flow. Rainfall increases in the first two summers were not duplicated the next two summers, so the experiment was modified slightly and continued for three more years. Battan (1966) and Battan and Kassander (1967) concluded that the data do not support the hypothesis that rainfall was increased.

In 1958, another randomized cumulus seeding project began in northeastern California, "to establish the extent to which the incidence of lightning-caused fires can be reduced by cloud seeding at a minimum cost

through use of regular field personnel." The study was initiated and supported entirely by the California Division of Forestry, which contracted with the U.S. Forest Service for assistance in design, operation, and evaluation. Seeding was randomized by 50% of the clouds being seeded and 50% of the clouds being unseeded during the nine hour period, 10 a.m. to 7 p.m. As indicated in the other two modification operations, no statistical significance was observed (Court, 1967). Similar inconclusive results came from other modification efforts during this decade, indicating the uncertainty and confusion about the physical understanding among meteorologists.

In 1958, federal legislation was passed that showed Congress was interested in maintaining and stimulating the field. Congress, in July of that year, gave the NSF authority to initiate and support a program of study, research, and evaluation in the field of WM. However, the NSF was given no statutory authority to direct, manage, or coordinate the growing government activities in this area. Fleagle (1969) notes this factor was one which caused policy arguments and later caused the NSF to be a center of dispute in the early and mid-sixties. Charak and DiGuilian (1974) note that two reasons for altering the NSF functions regarding WM were that the consequences of WM were broad enough to encompass far more issues than scientific ones, and progress in this area had reached the point where much developmental work, as well as continued research, was required. This uneasiness towards the NSF endured until 1968. In July of that year, Congress enacted legislation that curtailed the major role of the

Foundation in promoting scientific WM research.

Before considering some areas of research supported by the NSF and other agencies, we should examine the development of hail suppression, as this was the period in which it became important.

From 1949 through 1958, the number of hail suppression projects in the United States grew, and so did the land area of operations: from 400 square miles to almost 8,000 square miles (Koch and McGrath, 1973). During the fifties and sixties, projects were being conducted in foreign countries as well as the United States. Five of these foreign countries were the Soviet Union (Battan, 1969), France (Dessens and Lacaux, 1972, Picca, 1971), Germany (Muller, 1967), Switzerland (Schmid, 1967), and Argentina (Iribarne and Grandosa, 1965).

This large increase in interest of hail suppression activity is reflected in figure 3-4, the yearly totals of hail cloud modification experiments (1945-1975), and figure 3-6, the comparison of yearly totals of storm-system , warm fog, and hail cloud modification experiments (1945-1975). This increase is shown in these two figures by the large jump in reported projects at this time (1955-1965). The continued increase in hail suppression activities as indicated in figure 3-4 in the United States and elsewhere, stems in part from two reasons (noted below) as observed by Koch and McGrath(1973) and Battan(1965).

Koch and McGrath note that the large increase in the number of hail suppression projects was due to the fact that the results were accepted with extreme optimism and little statistical evaluation. The consequence of this optimism was that only one or two projects were

halted after the first year of operation. Battan points out that Soviet scientists have claimed spectacular hail suppression successes during the early sixties. These claims led to increased interest in hail modification. The Soviet claims have also resulted in U.S. scientists advocating a full scale, controlled hail suppression experiment (Jensen, 1976). Emanating from this recommendation was the National Hail Research Experiment (NHRE), whose objectives were discussed by Swinbank (1971) and Landsford (1976).

Further investigation of some experiments of this decade whose objective was to decrease hail damage has shown these experiments to have negative or inconclusive results similar to those of other modification projects (Atlas, 1977). These examinations as shown below, illustrate that the atmosphere needs to be defined better physically and statistically.

Two examples of additional investigation were Muller (1967) and Neyman and Scott (1967a). Muller found the results of the German project inconclusive because (1) an eight-year period was far from adequate for providing significant data about a weather phenomenon as infrequent as hail; and (2) the target area was too small compared with the variability of the weather situation.

Neyman and Scott (1967a) present two basic premises using data collected from five American experiments (SCUD, Whitetop, two from Arizona, and one from Washington-Oregon) and the Swiss experiment (Grossversuch III). These premises were: (1) there exist (at least) two sets of conditions, A and B, in which seeding has opposite effects

(precipitation is increased under A, decreased under B); and (2) these sets of conditions were identifiable in terms of the usual meteorological parameters (pressure, wind velocity, etc.). The problem, they conclude, then becomes the identification or the definition of the conditions; again, pointing out the need for better physical observation and understanding.

Finally, in May, 1965, the Interdepartmental Committee for Atmospheric Science recommended that the NSF should develop a plan for a collaborative effort in hail research. In response, the NSF formed Project Hailswath in 1966. The results of seeding indicated that although more hailstones fell on the target area, they were less damaging (Goyer et al., 1966, and Schleusener, 1967). Among the lessons learned from this project were the need for rapid recall and analysis of the observational data and the desirability of concentrating efforts on specific storms, rather than broad areas. This need to learn more about the atmosphere was characteristic of this period and reemphasized the necessity for better observations before results could be verified physically or statistically.

Returning to the mandate for research supported by the NSF and by others, we see that this research is well represented in the literature. AgI continued to share with other topics (listed next), a leading role in the number of articles reported; however the innovation and dispute on this subject of the previous decade no longer dominated the literature. Other topics considered included nucleation ability, physical design criteria, and organic seeding agents.

First, recalling the controversy during the first decade over the mechanism by which ice forms on AgI, one notes intensive work in this area. Birstein and Anderson (1955), Schaefer (1954), and Cwilong (1949) showed that ice can form at a relative humidity less than 100 percent and concluded that silver iodide was a sublimation nucleus. Contradictory reports were made by Fournier D'Albe (1949) and Mossop (1956), who both found that ice formed only when the relative humidity exceeded 100 percent and therefore held that one could not distinguish whether the process was one of condensation-plus-freezing or one of sublimation.

Edwards and Evans (1960) added to this debate when they showed relative humidity was important as an ice forming mechanism because AgI is a hydrophobic substance and requires for sublimation a supersaturation greater than that found in natural clouds. They conclude that there remains only one mechanism by which ice can form in atmospheric clouds-i.e., by the freezing of a cloud droplet with which a AgI particle has collided. Other topics of AgI were also considered for research.

This research about AgI varied widely. Fletcher (1959) and Bryant and Mason (1960) both did more work on the photolytic deactivation of AgI. They found, respectively, the decay rate depended on size distribution and trace impurities. Zettlemoyer, Tcheurekdjian, and Chessick, (1960) discussed surface chemistry. They reenforced earlier work by stating that the AgI surface was hydrophobic.

Edwards and Evans (1960, 1961) did work showing that the maximum

efficiency size of particles should be between 100 A° and 400 A° (1 Angstrom=10^{-8}cm). Also, it was found that by chemically adding to the AgI particles, the ice nucleating ability could be increased and extended (Burley and Herrin 1962; Rowland, Layton, and Smith 1964; and Koenig, 1964). Koenig also found that the acetone complex was a most favored complex as an ice nucleus. This research benefitted WM by defining more accurately processes not well understood, by producing more advanced and better developed operating equipment, and by making suggestions for evaluation techniques, both physical and statistical.

The following are some of these physical suggestions by scientists. Fletcher (1959) did calculations of maximum numbers of nuclei produced per gram of AgI and made suggestions for improving nuclei burners. Koening (1960) proposed a chemical test to determine the physical role of AgI. Elliott and Shaffer (1962) attempted to establish quantitative precipitation parameters for cloud-seeding evaluation. Braham (1964) declared one of the central problems in WM to be delineation of the climatological, seasonal, and geographic boundaries of an active rain process. Adderly (1961) suggested statistical evaluation techniques using non-parametric methods. Finally, Elliott **noted that** inasmuch as the nuclei supply was the one variable in the atmospheric outdoor laboratory over which man possesses a modicum of control, man could effectively increase his knowledge of cloud physics by varying this supply and observing and interpreting results. In the attempt to increase knowledge about cloud physics by controlling ice nuclei,

the ability of organic materials to act as ice-seeding agents was discovered and then pursued during the NSF mandate to fund research.

Head (1961) reported the discovery of a class of organic materials capable of acting as ice nuclei. His experiment on water insoluble organic compounds, the symmetry of which mainly was monoclinic (having one oblique intersection of the axes) or orthombic (having the axes at right angles to one another), showed that the compounds act as ice nuclei almost effectively as AgI does.

Actually, organic materials had been discussed previously in WM. For example, amino acid , an organic compound, had been identified in rain by Fonselius (1954) and Munczak (1960). Neither of these articles, however, suggested organic materials to have ice-nucleation abilities.

Power and Power (1962) explained the ice-nucleating ability and expressed optimism for this new class of seeding agents. They concluded, "at first sight, it did not seem likely that organic materials as a class would be active, since most of them are soluble in water, and water insolubility has generally been conceded to be a prerequisite for ice forming nuclei." They continued, "however, some amino acids are soluble only with difficulty in cold water and could possibly be successful agents (e.g., -OH, =O, $-NH_2$ groups)."

Related research efforts include the following:

Fukuta (1963) suggested that common organic compounds could overcome some faults (e.g., high price and photolytic decay) of AgI as a practical seeding agent. Research continues to the present in finding

other less expensive agents (Corren and Barnes, 1975) and into the problem of photolytic decay (Super, McPartland, and Heimbach, 1974).

Urea, another organic material, was observed to have extraordinary ice nucleating abilities in the laboratory and natural clouds by Knollenberg (1966). His laboratory experiments showed the nucleation mechanism involved high endothermic heat (a process where heat is absorbed by a substance from the environment) of solution and high solubility of urea. His field experiments in Wisconsin showed that urea appeared to be as effective as dry ice in causing shower formation.

Besides suggestions for physical design criteria, cloud physicists did experiments to define the atmosphere more accurately. Attempts to estimate precipitation efficiency were done on stratiform systems (Weickmann, 1957 and Wexler and Atlas, 1958) and in orographic situations (Elliott and Hovind, 1964 and Myers, 1962).

On a different topic, studies were done to see the relation between the time to grow precipitation particles and the lifetime of individual cloud cells. Braham (1958) found that in a study of convective radar echoes in Arizona, the time required for formation of precipitation was almost the same as the time of growth of the individual cloud cells. Braham (1964) found similar results in Missouri. Saunders (1965) concluded that simple single cell clouds have effective lifetimes about equal to the time to grow raindrops.

Along with the meteorologists' advocation for further research in the physical understanding of cloud processes, we find a trickle

of statisticians entering the field. The statisticians noted different consequences of modification and evaluation efforts. The following are some of the WM topics that were reviewed by them with some of their suggestions for improvement of statistical design and evaluation.

Linear regression was discussed by Moran (1956). He said it was not possible to fit a simple linear regression when both variables (e.g. total precipitation and total run-off) are subject to error. However, Moran noted that it is possible to identify bounds for the position and slope of the linear relation for this case.

The soundness of evaluation by the so-called "historical regression" method was debated by Neyman and Scott (1960). Neyman (1976) defined this methodology used in cloud seeding operations at this time in the following way. First, "consider two not very distant areas, one being the area in which the WM operator contracted to increase the precipitation, called "target," and the other called "control". The presumption was that cloud seeding over the target cannot possibly affect the precipitation over the control.

"Next, using precipitation data available for a few years before the beginning of the cloud seeding era, one established the linear regression line of the target precipitation, say $\underline{Y}$, on the control precipitation, say $\underline{X}$. Then, having the control precipitation for periods of actual cloud seeding, say $x_1, x_2, \ldots, x_n$, this regression equation is used to compute the precipitation in the target, say $\underline{Y}(x_i)$, to be expected without seeding. Finally, if $\underline{Y}_i$ stands for the target precipitation measured on the $\underline{i}$ th seeding operation,

the difference $\underline{Y}_i - Y(x_i)$ is treated as measuring the effect of seeding. At first sight this methodology may appear convincing. Closer examination of both meteorology and statistical application, appears to reduce its value."[2]

One meteorology problem is that there may be several "types" of storms (eg. frontal, squall, etc.), each with a different target-control regression line. The frequencies of these different storms vary from one year to the next. Thus, the historical target-control regression line need not coincide with that appropriate for the period of cloud seeding operations. Brier and Enger (1952) performed two evaluations of the same cloud seeding operation in Arizona. One analysis was done using a 10-year historical period, and the other using a 30-year historical period.

Figures 5-1 and 5-2 are the scatter diagrams showing the relationship between the seasonal precipitation amounts in the project area and the comparison area for the 10-year period and for the 30-year period, respectively (from Brier and Enger, 1952), showing that historical regression lines need not coincide.

Figure 5-3 (from Neyman, 1976) shows the change in yearly precipitation in Arizona over more than a quarter of a century, preceding the cloud seeding operations investigated by Brier and Enger. It is seen that over a period of some two decades the annual precipitation in Arizona showed an increasing trend. Subsequently, there was a period of five leaner years. This continued in the absence of any known human efforts to modify the weather. Thus, any difference between

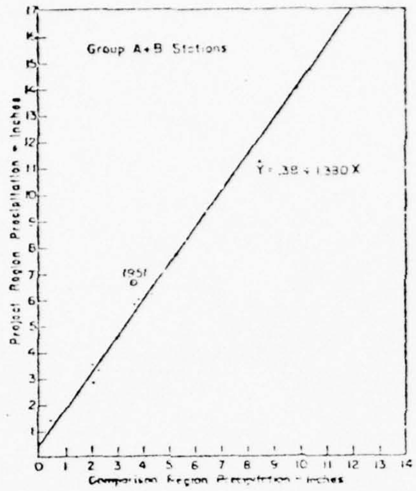

Fig. 5-1. Scatter Diagram showing the relationship between precipitation amounts in the project area and comparison area for a 10-year period, (from Brier and Enger, 1952)

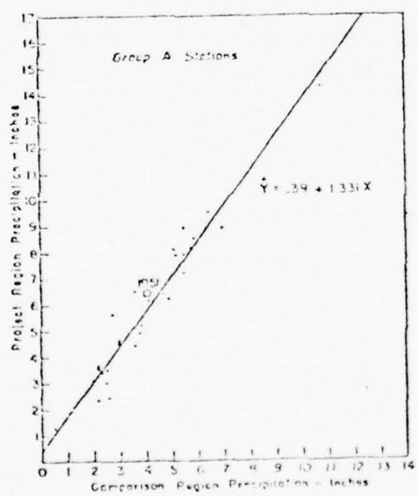

Fig. 5-2. Same as above except for a 30-year period, (from Brier and Enger, 1952)

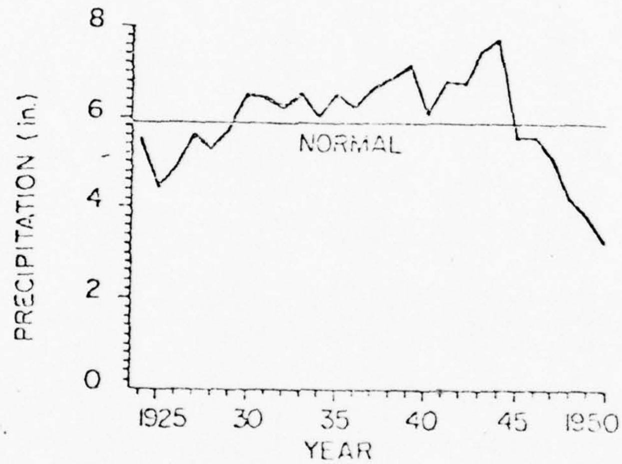

Fig. 5-3. Five year moving average of seasonal precipitation in Arizona (from Hayman, 1976)

rainfall in a subsequent period with cloud seeding, and that in some earlier period may well reflect a natural change in the precipitation patterns, and having nothing to do with seeding.

Another statistical problem is the bias in the historical regression-line method due to the use of transformations of the original data (Neyman and Scott, 1960). They note that when historical regressions are computed using transformed variables, the bias in question has always the same sign, favoring the conclusion that seeding increased the rainfall. They also discuss the second volume of the Final Report of the Advisory Committee on Weather Control and note that there were misrepresentations in the evaluations of cloud-seeding projects using this method in this report.

Since no authority existed to review evaluation methods, reviews like Neyman and Scott's were important in exposing faults in commonly used evaluation routines. The widespread use of the historical regression method (e.g., as reported by MacCready (1952); Hall, Henderson, and Cundiff (1953); Buell (1955); Siliceo, Perez, and Mosino (1963); Smith, Adderly, and Walsh (1963); and Henderson (1966)) particularly illustrated the need for investigations into the soundness of evaluation methodologies similar to Neyman and Scott's.

Two other topics examined by statisticians were selection bias and project design. Selection bias was discussed by Blackwell (1957). Project design was discussed by Brownlee (1960).

Blackwell's paper considers to what extent selection bias can be controlled through design of the experiment. He considers several such

designs in his article. Brownlee first comments that the historical method can produce fictitious results due to possible selectivity on the part of the seeding operator. Then he suggests a design in which he proposes that the operator observe two areas, with one or the other randomly seeded, thus giving an analog to a cross-over design (this design involves a single area that is seeded on a randomized basis and a nearby control area that is never seeded).

This decade of WM ended with a meeting by the NAS on the prospects of WM. Weiss and Lambright (1974) note that a consensus among the participants was reached at this meeting that there was promise for WM, a consensus that has grown within the scientific WM community, and is examined in the next section.

In summary of this decade of WM, we observe that the projects started earlier in the period initiated evaluation attempts. At about the same time these operations started, the federal government adopted statutes to maintain and stimulate the weather modification field. Areas studied in this decade include hail suppression, ice nucleation ability of both organic and inorganic materials, as well as studies by cloud physicists into precipitation efficiency and the relation between raindrops and cell growth. Important by-products of this research were the conclusions by those involved in WM, both scientists and statisticians. These people concluded that the physical models and statistical considerations employed, failed to take into account that seeding could have a variety of effects which could be positive, negative, or have no bearing on precipitation (Davis and Hosler, 1967).

The call by meteorologists and statisticians for more physical understanding and better statistical design, and the inability of the NSF to regulate WM policy all contributed to tension and debate during this period. Disagreements became accentuated in the early sixties and resulted in two events: the first was to exclude the NSF from major scientific funding of WM; and the second was to reexamine the directions in which WM was moving.

This reexamination, both statistical and meteorological, as detailed in the next section, helped to reorient the entire WM endeavor.

5c. The Third Decade (1966-1975)

The third decade (1966-1975) of WM was marked by meetings held on the statistical and meteorological aspects of WM. Research in WM continued during this period, but was initiated and oriented in different directions, as for example, the initiation of comprehensive research into the social, legal, and economic ramifications of WM. The federal government maintained interest in WM by passing legislation requiring reporting of WM operations. Also some state governments entered the WM field. Finally, some suggestions for future work on WM by scientists associated with the field can be noted.

Hosler (1974) noted that WM research could best be characterized as field experimentation with varying degrees of statistical design, statistical control, and subsequent analysis. He observed that this research apparently had reflected a hope that much of the complex cloud physics and dynamics could be overlooked and that the effect of seeding clouds would be so large and unidirectional as to override the variations between individual clouds in a given location. Both statisticians and meteorologists looked for answers to these and other problems associated with WM.

Questions and problems about statistical schemes, control, and analysis of WM field experiments, plus some unawareness of WM studies by the Statistical Laboratory, University of California, Berkeley, California, led to an effort by this group to learn what

was going on in WM, both in the United States and abroad.

This effort resulted in a meeting in late December 1966 through early January 1967. Attending were a large number of American participants and eight foreign participants. The Proceedings of this meeting indicate the wide range of topics covered at this meeting and contain three sections on WM; one part has day-by-day data on as many randomized experiments as the group managed to assemble ("A Collection of Observational Data", 1967); another has reports on the group of experiments in the United States and foreign countries; and another has papers on WM methodology.

Reports of early experimentation in the United States and foreign countries at this meeting by geographical areas were the following: Arizona (Battan and Kassander, 1967), France (Beimer, 1967), Missouri (Decker and Schickedanz, 1967), California (Eberly and Robinson, 1967), Israel (Gabriel, 1967), Colorado (Grant and Mielke, 1967), Bavaria (Muller, 1967), Switzerland (Schmid, 1967), Mexico (Siliceo, 1967), and Australia (Smith, 1967).

Papers on WM technology given at this meeting were the following: problems in evaluating cloud-seeding effects over extensive areas (Brier, Carpenter, and Kline, 1967), the effect of natural rainfall variability in verification of rain modification experiments (Changnon and Huff, 1967), some techniques of summary evaluations of several independent experiments (Davies and Puri, 1967), tracking silver iodide nuclei under orographic influence (Henderson, 1967), on Pitman efficiency of some tests of scale for the gamma distributions

(James, 1967), physical factors of cloud seeding (Neiburger, 1967), the relationship of WM experiments to other areas of statistical application (Yates, 1967), and evaluation of WM as expressed in streamflow response (Yerdjevich, 1967).

At this meeting, Davis and Hosler (1967) expressed what they believed were upsetting aspects of meteorological design. They observed that no one experiment can be designed to give conclusive answers concerning all the benefits (or liabilities) to be derived from weather control by cloud seeding because the complexity of the microphysical, mesoscale, and macroscale interactions is too great. Two causes of this complexity they noted were that in a given field of clouds, using the same seeding techniques, different results could be expected depending on the size of the clouds and the existence of stable layers in the environment, and that it was important to realize that different seeding techniques may cause different effects on clouds depending on <u>when</u>, <u>where</u>, and <u>how</u> much glaciation occurs.

From the wide range of topics discussed at this conference, it was seen that the evaluation of a WM operation was difficult and no single statistical or physical design would suffice. Some research was directed at solving these and similar questions concerning WM operations.

The statistical research of the late 1960's and early 1970's was aimed at developing tests to evaluate WM programs, investigating selection bias, and answering certain physical questions that rose from statistical analysis of modification data.

Some of the tests for WM evaluation examined during this period were the sum of squared rank test (Duran and Mielke, 1968), optimal asymptotic tests (Kulkarni, 1968, 1969), and tests for the scale parameters of two gamma distributions using the generalized likelihood ratio (Schickedanz and Krause, 1970).

Duran and Mielke (1968) investigated the robustness of the sum of squared ranks test. They stated their purpose was to show that the unmodified sum of squared ranks test is reasonable to use when compared to the locally most powerful rank test for a number of specific asymmetrical distributions which have total mass confined to the positive axis (eg. rainfall).

Kulkarni (1968, 1969) considered two cases of asymptotic tests. He suggested that by using a general randomized design and determining the locally asymptotically most powerful tests, one can conclude that the crossover design (for a fixed effect) and the randomized design with noncontrolled predictor variables (for a variable effect) turn out to be special cases of general design.

Studies like these helped to increase the use of some form of the rank sum test as a statistical technique for evaluation as well as the proliferation of the crossover scheme for experimental design (Hanson, Bach, and Cooley, 1976). Later investigations during this decade would show the inadequacy of the crossover scheme.

Finally, Schickedanz and Krause (1970) consider a test that gives a more powerful result than the t-test. They develop a test between the scale parameters of two gamma distributions with common shape

and compare its power with that obtained by applying the t-test to non-transformed and transformed data. They conclude that the likelihood ratio test for differences in gamma-scale parameters is more powerful than the t-test applied to log-normal means.

On a different topic, Stigler (1969) considers selection bias. He proposes a design that has a maximum risk only slightly higher than that of a truncated binomial to reduce selection bias for meteorological research on WM.

It is important to examine the work done by Neyman and associates at Berkeley due to the relatively large volume and impact of their work. The Statistical Laboratory at Berkeley concentrated on projects "Grossversuch III" (Schmid, 1967), "Whitetop" (Grant and Mielke,1967; Grant et al., 1968; and Mielke et al., 1970), and the project in Arizona (Battan, 1966; and Battan and Kassander, 1967). Their efforts resulted in two "sets" of papers, one set on the first two projects and the other set on the Arizona project.

Studying the Grossversuch III data, this group found questions about the physical situation they could not answer. Neyman (1976) summarized these questions as:

1. Are there any signs that cloud seeding over a target of conventional size affects the precipitation in localities 100 miles away?

2. Does the seeding under "warm" stability layers tend to increase the precipitation, at least in proper orographic conditions?

3. Does the seeding of summer cumulus clouds, performed in conditions of uninhibited updrafts, tend to decrease the precipitation?[3]

The only way to obtain answers, they believed, without waiting for the organization and completion of appropriate new experiments, was to use other projects for which reliable data had been published.

The first effort by the Berkeley group to answer these questions was aimed toward the Whitetop experiment. Their discussions of these two projects (Grossversuch III and Whitetop) are found in the following papers: Neyman, 1967; Neyman, Scott, and Wells, 1969; Neyman, Scott, and Smith, 1969; Neyman, Lovasich, Scott, and Smith, 1969, 1970; Neyman, Lovasich, Scott, and Wells, 1970; and Lovasich, Neyman, Scott, and Wells, 1971.

However, as noted in these papers, the data were not suitable to answer the above questions because of the lack of radiosonde information and because the cloud seeding had been done by plane, at a height which was likely to have been above the "warm" stability layers, thus making it highly unlikely that they would be able to answer the second question about increasing precipitation. Furthermore, the last paper documents the conclusion that there must have been an important flaw in the implementation of randomized seeding and thus no reliable appraisal of the effect of seeding for the Whitetop experiment was possible. Subsequently, they concentrated efforts on the Arizona projects as noted earlier.

To answer the three questions posed above, Neyman et al. (1972, 1973) examined average precipitation in locations in Arizona. They found that at distances of 90 to 180 miles downwind from a target, on particular experimental days, there had been losses

of rain ascribable to seeding that averaged 34%, with two-tail significance probability P = 0.028. Thus, they believed that they had data to investigate further the first question.

The seeding in the Arizona project was for the first four years at the -6C level and for the last three at the base of summertime cumuli. They note that these levels are above most warm stable layers. They believed that this cloud seeding could furnish answers to the third question about seeding on days with uninhibited updrafts.

To obtain an answer to the question about decreased precipitation at long distances from the target and in conditions of large updrafts, Neyman (1976) discusses and illustrates a hypothetical mechanism for losses of rain ascribable to seeding, (figure 5-4), which was first proposed in 1971 (Neyman and Osborn, 1971). Their proposed mechanism suggests that when AgI smoke reached the cloud base, it stimulated rainfall. Then the rain falling through the air below the cloud, they observed, must have cooled a "parcel of air." Finally, they noted the cooled "parcel of air" as it drifted through the atmosphere, "killed" the updrafts and rain nearing the ground at far away distances. This mechanism was proposed by statisticians with the hope of stimulating cloud physicists to do better (Neyman, 1976)!

At first glance this mechanism appears sound due to its simplicity. However, questions become apparent with further study. Would the parcel of air maintain its identity in the atmosphere with the effects of entrainment (mixing) of environmental air into the parcel's air mass over a period of six hours? Even if the parcel of air did maintain its

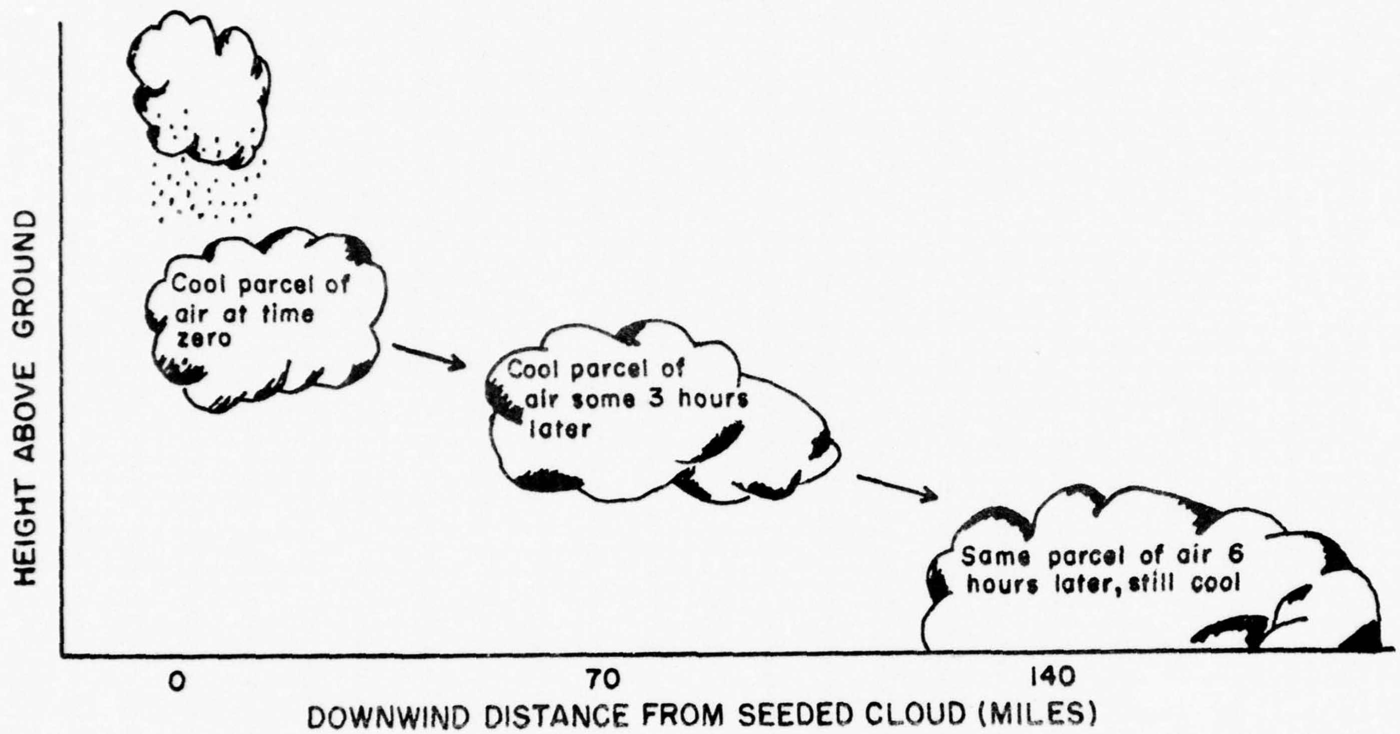

Fig. 5-4. Hypothetical mechanism of downwind far away negative effects of seeding at the Arizona experiment (from Neyman, 1976).

character for this period of time, would it "bump" the ground as indicated in Fig. 5-4? Obviously more work is needed in this area!

One other point discussed by Neyman (1976) is that due to the effect of seeding on a large scale (Neyman and Osborn, 1971), the crossover design must be abandoned. Other similar research showing cognizance of downwind effects is the following: Adderly, 1968; Brown and Elliott, 1968; Schickedanz and Huff, 1970; Schickedanz, 1974; and Janssen et al., 1974. This awareness of large area effects has been reflected in the use of experimental design. The crossover scheme has been used only a few times in this decade and the majority of operations have changed to a two-sample-use control plan or in the case of cumulus clouds, single cloud seeding (Hansen, Bach, and Cooley, 1976).

Statisticians were not the only ones to recognize the value of conferences. The A.M.S. has sponsored or cosponsored a conference on WM at least every two years this decade beginning with 1968. Conferences on WM were held in Albany, New York in 1968, in Santa Barbara, California in 1970, in Rapid City, South Dakota in 1972, in Fort Lauderdale, Florida in 1974, and in Boulder, Colorado in 1976.

The objective at the First Conference held in Albany, New York, was to provide an opportunity for meteorologists and engineers to present analyses of their experimental and theoretical work dealing specifically with on-going problems in weather and climate modification (1968, BAMS, 49, p. 525). The goals of later conferences followed the same pattern but included other topics such as warm fog

modification, orographic systems and their hydrometeorology, convective cloud modification, water management, and policy and social considerations.

Some other meteorological investigations have been about the recognition of a cloud-seeding opportunity (Grant, Chappell, and Mielke, 1968; Spar, 1968; and Riggio and Carr, 1974); studies into macroscale, mesocale, and microscale interactions of WM (Ruskin, 1967; Brown and Elliott, 1968, 1972; MacCready and Baughmann, 1968; Goyer and Wood, 1972; Hobbs, 1975; and Hobbs and Radke, 1975).

Continued study into the development of the physical understanding has taken place. However, in recent years the specializations of cloud microphysics, cloud dynamics, laboratory and theoretical cloud modeling and radar meteorology have become so interrelated that they can hardly be considered as separate disciplines (Cotton, Jiusto, and Srivastava, 1975).

Some of the research has been concerned with nucleation and ice generation in clouds. Under the topic of general nucleation, there have been studies about the thermodynamic treatment of homogeneous condensation (Reiss, 1970; and Lee et al., 1973) and the ice nucleation behavior of AgI smokes (Fletcher, 1968; and Layton and Steger, 1969).

Fletcher (1970) has clarified earlier theoretical work on ice nucleation activity. He points out that the earlier treatment generally does not apply because it was based on ice nucleation by sublimation on homogeneous surfaces which are now characterized as being heterogeneous. He notes that an ice nucleus surface is not uniform,

but is better characterized by a number of active nucleation sites: hydrophilic, impurity, adsorption, and electrical. Thorough understanding of the ice phase in clouds, from nucleation to crystal development, was recognized as a high-priority item by a NSF-AMS cloud physics panel (Braham and Squires, 1974).

The use of AgI in WM has continued to be seriously investigated. Some of the areas of research are the design and development of AgI generators for WM (Steele, Davis, and Procter, 1970; Paik, Fukuta, and Todd, 1972; Henderson, 1972b; and Parungo, Ackerman, and Pueschel, 1974), the investigation of complex AgI ice nuclei (Burkardt and Finnegan, 1970; Chen, Davis, and Johnson, 1972; and Henderson, 1972a), and the detection of AgI ice nuclei (Langer, 1970; Alkezweemy, 1970; and Parungo, 1972).

Organic materials also were further examined for use as nucleating agents this decade. Fukuta et al. (1966) suggested using metaldehyde for nucleating ice in supercooled clouds. Other work has been in the effectiveness of liquid propane for use as a fog dispersal agent (Gerdell, 1968; Palmer and Smith, 1973; and Hicks and Vali, 1973). On a different topic, Fukuta, Armstrong, and Gorove (1974) discuss the development of organic ice nuclei generators for WM.

Other meteorological research has resulted in suggestions for physical design aimed at the dynamic effect of cloud seeding. MacCready and Skutt (1967) noted that when supercooled clouds were completely overseeded so that all the final condensate was ice, there was a buoyancy-increase effect. They believed that this buoyancy-increase

was of special importance in some cases because it should lead to significant changes in cloud dynamics. Further study and experimentation with dynamic cloud modification is discussed by Simpson (1967), Tribus (1970), and Woodley (1970).

Cloud physicists were also involved with WM this decade. For example, Houghton (1968) re-examined the natural precipitation processes on the basis of accumulated knowledge of the microphysical aspects and field observations, with particular attention to the implications for cloud seeding. He concluded that opportunities exist for the modification of convective, storm-system , and orographic precipitation, but such opportunities occur only under certain specific conditions and at particular times. He further notes that additional evidence is required in order to establish the extent to which such favorable conditions exist.

This need for closer monitoring of the actual physical processes in and around the clouds has been noted by many meteorologists. Three of the more recent papers on calls for research oriented toward providing better observations are Brier (1974), Hosler (1974), and Atlas (1977).

Before moving into aspects of the social, economic, and legal sides of WM, it is prudent at this point to discuss figure 3-3, the yearly totals of warm fog modification experiments, because of the large increase of articles at this time.

There are two probable reasons for this large increase. First, an economic one, is that in 1969 the Navy increased its expenditures

threefold as compared to those of previous years, for warm fog modification research (Koch and McGrath, 1973). The second is that the Second National Conference on WM at Santa Barbara, California, held during April 6-9, 1970, devoted a whole session to fog modification. This resulted in the publication of articles that obviously would have appeared only in technical journals (see, for example, Silverman, 1970; Kunkel, 1970; and Hilsenrod, 1970).

In addition to the scientific research of this time, initiation of serious, comprehensive study into the social, economic, and legal ramifications of WM occurred (Frazier, 1970; and Changnon, 1973). Mordy (1975) notes that the guardedly optimistic report by the National Academy of Sciences in 1966 prompted scholars in the social sciences to look at the implications of WM on society. Studies were undertaken and symposia held to explore the social, economic, legal, political, and ecological implications of WM (Sewell, 1966; Fleagle, 1969; and Taubenfeld, 1968, 1970).

Further calls for research on these topics were made by Davis and Hosler (1967) and Sewell (1968). Davis and Hosler believed that if this country were to embark on a full scale program of investigating the potential of WM, it seemed necessary to launch studies of economic, social, and legal implications of WM. Sewell (1968) noted that three major problems had given rise to growing public concern about the atmosphere and needed further study were: (1) the volume of air pollution; (2) the rising losses due to extreme weather events; and (3) man's increasing ability to modify the weather.

Research in the social areas has been channeled down different avenues. Three of these are: (1) the effect of social setting on WM efforts (2) studies of citizens' response to WM; and (3) the influence of the general public on the control of WM projects.

Two examples of social setting that have led to conflict concerning WM efforts have been in the states of Florida and Colorado (Haas, 1973). In Florida, a severe drought in late 1970 and early 1971 in the central section of the state led to conflict of economic interests among the residents in this region when the Florida Rain Augmentation Program operated during the period, April-July, 1971. Tomato and melon farmers in this region knew that large amounts of rain about harvest time could cause their crops to rot. Beef farmers, citrus growers, and others suffering from the drought would not be hurt. The difference in economic interests led to arguments and disagreements, but no altercation happened during the operation in Florida. The only settlement was to a person who claimed that hail caused by the seeding broke his car's windshield.

Another example noted by Haas was the Augmentation and Suppression Program run in the San Luis Valley, Colorado in the late sixties and early seventies. A conflict arose here when barley growers hired a commercial firm to produce additional rain and suppress hail. Opposition to cloud seeding came from lettuce and potato growers and other farmers, who believed the modification program was reducing the amount of rainfall they need. Feelings ran very strong and in 1972 the weather modifier's trailer was bombed. These examples of conflict

illustrate how social settings must be considered in developing and operating a WM project.

Another topic of social research about WM has been the problem of citizens' response to WM. Mordy (1975) lists the important variables observed from studies of the responses as the following: (1) the history of the cloud seeding in the area; (2) the degree of heterogeneity of weather needs in the target area; (3) the involvement of local government. He also observed that there are indications that most people accept WM research provided that it is not perceived as a threat to their economic interest.

The public acceptance of cloud seeding in South Dakota has been investigated by Farhar (1974). She has shown that the majority of variance in the public's evaluation of the WM program in this area can be explained by attitudes toward WM, belief in efficacy, knowledgeableness of WM, and religio-natural orientation (cloud seeding probably violates God's plan for man and the weather).

The final topic of social research examined in this study is the possible influence of what the public thinks about WM and how this possible influence could affect or control WM. The following two quotes illustrate concern on how public opinion could affect WM:

> from Prof. Charles Cooper of San Diego College, an ecologist:
>
> "I predict that WM will be one of the first technologies over which the general public, rather than the scientists who devised the technology and the economic interests who see immediate benefits, will exert control."[4]

from an ICAS report

"What the public thinks about WM, rather than what the scientists know about it, will play the dominant role in the future of this science. The most expertly developed technology, whether it be for augmenting water or for suppression of damaging weather-phenomena, will find only limited application in the absence of a strong public demand."[5]

Heeding the above warnings, scientists made suggestions for communication, both formal and informal, between the public and the meteorological community . Lansford (1973) suggested with farmer groups, with Lions or Rotarians, or with county Agricultural agents; or the use of daily or weekly newspapers to increase communications between the two groups. Farhar (1974) has observed that most information about cloud seeding was disseminated through informal informational networks (meetings, word of mouth, etc.) in each community and through the local written media, thus reinforcing Lansford's suggestions. Concurrent investigations on the topics of economics and law concerning WM have been made.

Table 5-1, (Summary of United States-Sponsored Weather Modification Research ($)), was constructed from data given by Koch and McGrath (1973) and Jensen (1975). This table shows funding for WM research by the federal government increased steadily during the sixties and in the early seventies, and jumped to a peak in fiscal year (FY) 1972 of $19,800,000. After level funding in FY 1973, support for the program decreased approximately 22% to $15,300,000 in FY 1974 with the same level of funding in FY 1975 (Jensen, 1975).

TABLE 5-1

Summary of United States - Sponsored Weather Modification Research ($)

Year	Amount
1946 - 1958	$ 4,000,000
1959	1,750,000
1960	2,100,000
1961	2,420,000
1962	4,570,000
1963	2,750,000
1964	3,530,000
1965	4,970,000
1966	7,030,000
1967	9,910,000
1968	11,300,000
1969	11,590,000
1970	12,930,000
1971	15,000,000
1972	19,800,000
1973	19,800,000
1974	15,300,000
1975	15,300,000

Sources-Koch and McGrath,1973; Jensen,1975

This decrease has been examined by Changnon (1975). He believes a paradox has developed involving the sizeable reductions of federal support compared to major scientific-technical advances in the field. Four developments are cited by him. They include capabilities (1) to dissipate cold fogs (AMS, 1973; and Charak and DiGuilian, 1974); (2) to enhance snowfall from orographic clouds (Grant et al., 1971; and Mielke et al., 1970); (3) to increase rain from tropical clouds (Simpson, 1970; Simpson et al., 1971); and also (4) discovery of sizeable urban-related increases of rainfall (Changnon, 1968, 1969; Huff and Changnon, 1973; Sanderson et al., 1973; and Schickedanz, 1974).

He lists the following as basic or external reasons for reductions of federal support:

basic:
1. immature technology
2. the socio-economic impacts are ill defined
3. uncertain management

external:
1. general lowering of the national image of science and the resulting reduction of growth funding for all of science
2. the diversion of funds elsewhere to support research related to major crises (e.g., energy) or to support growth in long-term commitments for other less controversial atmospheric research programs such as Global Atmospheric Research Programs
3. desire of executive branch of government to involve local and state support, rather than federal support, and to have commercial enterprises, rather than federal agencies performing research and applying technologies as much as possible
4. general federal cautiousness to uncertain sciences and controversies.

Other related studies on WM have been: the economic benefits of fog modification (Hilsenrod and Hermie, 1970); the monetary benefits of WM (Burke and Kriege, 1972); the estimation of potential economic impact through use of simulation models (McGuigg, 1970); the effects of weather variables on the prices of Great Plains cropland (Borland and Snyder, 1974); and the effectiveness and potential of precipitation processes in the Connecticut River watershed (Spiegler and Aubert, 1970).

Investigation concerning the legal aspects of WM indicates how very little has been done on this topic at the federal, state, and international levels. At the federal level, the only directly pertinent law was passed in December, 1971. It requires WM activities to be reported to the Federal Government, but no provisions are made for regulation or control of WM (Charak and DiGuilian, 1974). In February, 1974, an amendment to the original law was passed with new rules requiring the reporting of current safety practices and environmental considerations associated with a WM project. Also, the responsibility for design and operation of this reporting system has been delegated to the National Oceanic and Atmospheric Administration (Charak, 1976).

At the state level, one finds that thirty states regulate WM activities in some manner (Droessler, 1975). Provisions commonly found in state laws concern establishment of boards and commissions, license requirements, penalties for noncompliance with the laws, permits for specific operations, the financial responsibility of operators for results of modification, and requirements for records and reports

(Mordy, 1975). Finally, there has been a suggestion in state statutes of a developing trend toward increased comprehensiveness in WM legislation accompanied by more extensive provisions for public involvement in decision making (Farhar and Mewes, 1974).

On the international level, very little has been done (Weiss and Lambright, 1974). Figure 5-5, shows the nations in which weather (precipitation and hail) modification projects (experimental or non-experimental) have occurred since 1945 (from Changnon, 1975), and clearly illustrates the need for such activity because of the possible international effects of WM operations.

Finally, suggestions for future work in WM are varied. Some of these are: the call for a well-defined national program of WM activity (Droessler, 1975; Changnon, 1975); the development of a better physical design by incorporation of satellite information (Dumont et al., 1974) or by use of three-dimensional mesoscale models for prediction and analyses (Cotton and Pielke, 1976); for further emphasis on research on inadvertant modification (Jiusto, 1974; Mordy, 1975); and for further statistical work in hypothesis testing and experimental design, and a better methodology for extended area analyses (Julian and Murphy, 1972). Among these suggestions, however, one finds the need of some form of catalyst, as noted below, to stimulate research efforts (Sewell, 1968; Haas, 1973; Weiss and Lambright, 1974; Changnon, 1975).

Possible catalysts for WM suggested by the above people include a major weather-related problem (e.g., severe drought), or a major claim or a scientific break-through in WM by a foreign country; one that would

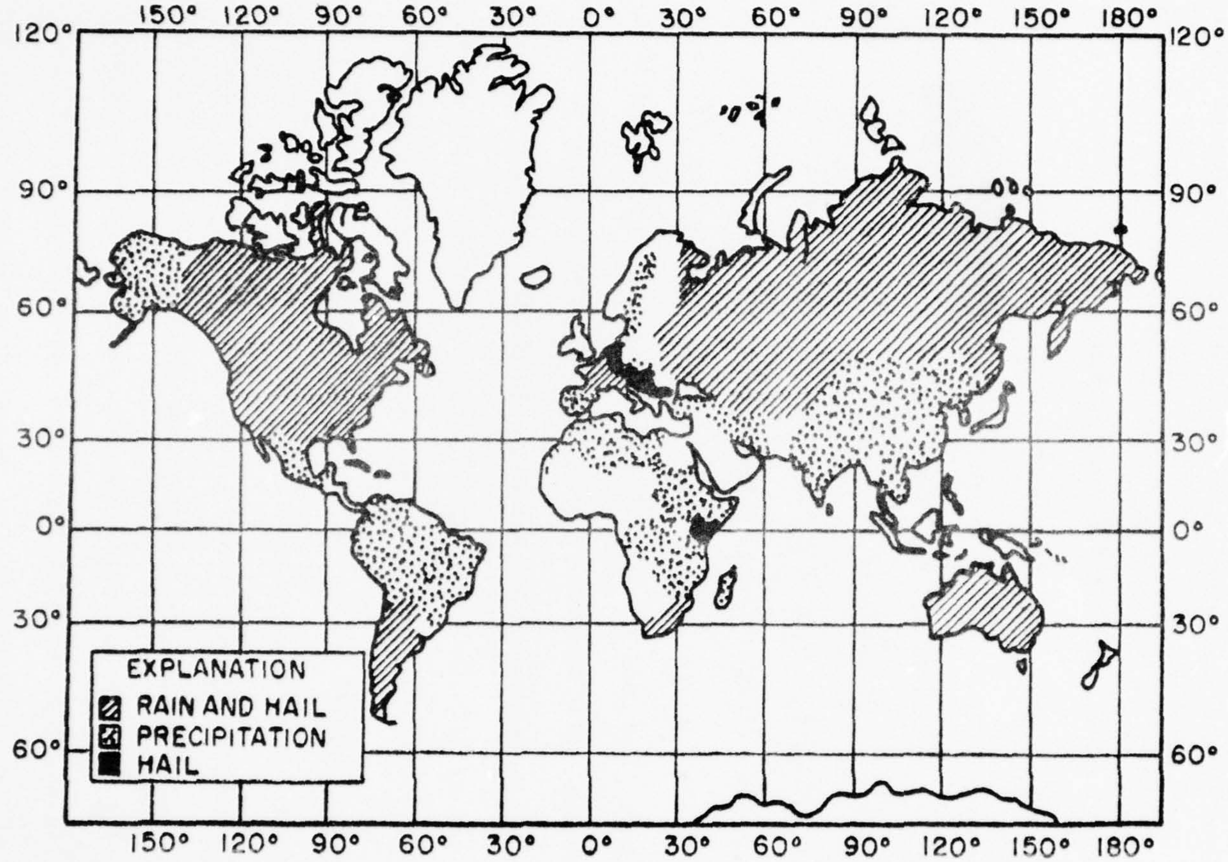

Fig. 5-5. Nations in which weather (precipitation and hail) modification projects (experimental or non-experimental) have occurred since 1946 (from Changnon, 1975).

have sizeable scientific acceptance and public attention, particularly if coupled to findings allowing assessment of the socio-economic change.

To summarize this decade, we see a changing attitude toward WM. This is reflected in the different topics of research and in the exchange of results and ideas at statistical and meteorological conferences. Both meteorologists and statisticians have recognized the need for thorough investigations of the physical conditions before more sophisticated designs can be developed. The importance of public opinion to influence WM was noted. Serious research was initiated in this period on social and economic topics. Governmental action in the form of laws has been slow but there seems to be a trend toward increased comprehensiveness in the laws with provisions for public involvement. Lastly, some suggestions are noted for further research and stimulation.

6. Summary and Conclusions

The 30-year period (1955-1975) of WM has been reviewed. Emphasis has been placed on examination of WM literature, on the development of the physical understanding, and on the evolution of both statistical and meteorological design criteria for WM experiments. Also, social, economic and legal aspects of WM are studied as they affect the above.

Five particular topics were looked for in the examination of the literature. They were: 1. storm-system modification, 2. hail modification, 3. warm fog modification, 4. cold fog modification, and 5. lightning modification. Examination and comparison of the totals of articles on these topics showed the first three (storm-system, hail, and warm fog) were the most numerous. Further chronological analysis of these topics showed they became prominent in WM history.

Following a brief review of WM attempts before 1930, an investigation of the three decades of WM was made. This investigation has shown that the first decade (1946-1955) was dominated by scientific innovation and dispute of physical concepts, legislative inaction, and absence of statistical design criteria. The second decade (1956-1965) featured statistical and physical evaluation efforts aimed at ending the controversies of the first 10 years, further innovation of physical concepts, and saw the federal government take interest in WM. The third decade (1966-1975) showed a new direction of movement in WM. Examples of this

were the interchange of ideas and information at conferences and symposia, research directed toward different topics such as the social, economic, and legal problems of WM and the entrance of state and local government in WM.

Along with the new direction of movements during the third decade came assessments by those involved with the WM field. These evaluations noted reasons for problems in WM as well as suggestions for future work.

Houghton (1968) notes some of the difficulties encountered in evaluating a WM project may be attributed to inadequate experimental design, too short a series of experiments, the large variability of precipitation, and to the failure of the experimenters to insert the seeding material in suitable concentration into proper regions. Brier (1974) observes that difficulties in experiments have been due to high natural variability, evidence that seeding effects are both positive and negative, and error in instrumentation and measurement.

Along with these assessments have come suggestions for future experimentation. Hosler (1974) concludes that progress in cloud seeding is going to depend very highly on our ability to predict the sequence of events in small-scale phenomena such as cumulus clouds and cloud groups, and this involves the construction of sound physical models. He further notes that the parameters (e.g., rates of coalescence, rates of ice formation, rates of entrainment, etc.) of these models will emerge from very detailed and careful laboratory experiments and carefully made measurements in clouds by aircraft. Atlas (1977) has

suggested that in order to enhance the chances of success of a statistical experiment on hail, a scheme of stratification which would permit the physical discrimination between increased and decreased hail should be developed. He also notes that the strength of a statistical experiment would also be enhanced and its duration reduced by the use of a strong covariate such as dynamic hail potential(the combination of maximum updraft velocity and temperature at the same height to give an indication of maximum hail size).

In conclusion, the value of this report is fourfold. First, it fills a need for relating the development of physical and statistical topics of WM. Secondly, it presents a summary of the number and chronology of five areas of research of WM experiments. Thirdly, is the collection of over 250 references on nearly all aspects of WM. Fourth and finally, is that it shows that WM research has occurred in steps(with storms, hail, and warm fog being the individual steps), and thus leads one to extrapolate that the next decade will bring us another step. Jiusto (1974) has suggested that perhaps this will be inadvertent weather modification.

FOOTNOTES

1. Fleagle, R.G., 1969, "Weather Modification: Science and Public Policy", University of Washington, Press, Seattle, Washington, pp. 6-7.

2. Neyman, J., "Experimentation with Weather Control and Statistical Problems Generated by It", Symp. on Applications of Statistics, Dayton, Ohio, pp. 5-6.

3. Ibid, pp. 27-28.

4. Landsford, H., 1973, "Weather Modification: The Public Will Decide", BAMS, 54, p. 658.

5. Ibid, p. 658.

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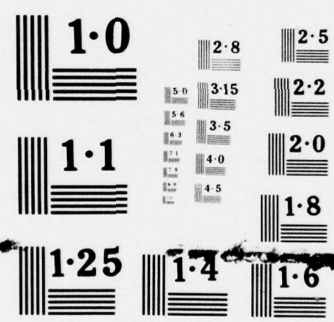

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20. ABSTRACT

The development of the physical understanding of weather modification and the evolution of statistical and meteorological design criteria for weather modification experiments for the 30-year period (1945-1975) are investigated. Also, social, economic, and legal problems of weather modification are discussed as they affect the above.

Graphs are constructed depicting the chronology of reported articles on storm-system (in this paper 'storm-system' refers to attempts to increase rainfall from extratropical cyclones and organized systems of clouds), cold fog, warm fog, hail, and lightning modification. An attempt is made to explain the changes in the number of experiments reported during the description of the evolution of weather modification.

The summary of the 30-year period of weather modification is for three 10-year periods. The first decade is shown to be dominated by scientific innovation and dispute. The second decade featured initiation of efforts by meteorologists and statisticians at ending the controversies of the first 10 years. The third decade was marked by increased exchanges of ideas and results of weather modification operations at conferences and symposia and serious investigation into social, economic, and legal ramifications of weather modification.